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THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

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JOHN LESLIE, Professor of Natural Philosophy in the University of Edinburgh, was born at the village of Largo, in the county of Fife, on the 16th of April 1766. If, in sketching the Life of this eminent person, we should happen to exceed the usual limits of our biographical notices, we can at least plead in excuse that we have done so in the case of one of the greatest benefactors of this Encyclopædia,—one whose counsels led to not a few of the improvements which distinguish the later years of its history, and to whom it is indebted for many contributions, marked with the stamp of his vigorous and original powers. It is painful to us to recollect the number of similar claims to our attention. Too many illustrious contemporary benefactors have, like him, left us the mournful duty of preserving some memorials of their personal history, in the work which received from their co-operation its highest honours and proudest recommendations.

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* This Memoir was written for, and published, pretty nearly in its present form, in the seventh edition of the *Encyclopædia Britannica*, of which Mr Napier is Editor. Of this edition fifteen volumes have already been published.

He was the son of humble, but, in their line of life, highly respectable parents, who lived to enjoy the celebrity of their son, and for whom he ever cherished that affection which formed a marked feature of his character in regard to all the members of his family. His father, originally from the neighbourhood of St Andrews, lived for some time at Anstruther, but ultimately settled at Largo, as a joiner and cabinet-maker. His wife, Anne Carstairs, was a native of that place; and the subject of this Memoir was the youngest child of their marriage. Though he attained in manhood a robust frame, he was, in early youth, of a very feeble constitution; so much so, that when sent, at four years of age, to a sort of school, kept by an old woman, who plied her wheel whilst teaching the alphabet, he was indulged with a separate stool near the fire-place, which the dame set apart for the feeblest of her juvenile pupils. As long as he was permitted to monopolize this seat of honour, he seems to have been tolerably pleased with his situation; but being at length superseded by a younger or more favoured pupil, he eloped from the school, hid himself for a day in some obscure corner, and, when obliged to come forth, obstinately refused to return to the tutelage of the ancient spinster. He was in consequence placed in another school, where he remained six months, and was taught writing and arithmetic; his father and his eldest brother, who appear to have possessed some knowledge of the elementary parts of mathematics, giving him, at the same time, his first lessons in that science. He was afterwards entered at a school in the neighbouring town of Leven, where Latin was taught; but his intense dislike to that language, and his inability, from weakness, to walk and return daily a distance of three miles, induced his parents to discontinue his attendance, after a short trial of six weeks. Such was the brief and meagre *curriculum* which formed the whole training of the future Mathematician and Philosopher, previous to his being entered a student at the University of St Andrews! But we must not too hastily despatch this early period of his life, when his genius, working with its own inward resources, already began to attract observation.

The first person of any sort of distinction who noticed his precocious attainments, was Mr Oliphant, who became Minister of Largo about the time when the boy had reached his eleventh or twelfth year. Struck with the knowledge in mathematical and physical science which he displayed, the reverend gentleman kindly lent him some scientific books, with which he was but poorly provided; and he also strongly urged him to study Latin—telling him, by way of showing the necessity of an immediate commencement, that it had cost himself seven years to acquire that language. This was the worst argument that could be urged to the young philosopher, who unhesitatingly declared that he never would bestow half that time upon any language, and that he particularly disliked Latin. In this state of his knowledge and taste, he was, in his thirteenth year, sent to the University of St Andrews to study mathematics under Professor Vilant. On examination by the Professor, he was found already qualified for the second or senior class; and, at the close of the session, he obtained a prize. It is remembered, as a characteristic particular, that having previously discovered, in some of those antiquarian researches to which he was early addicted, that it was not indispensible for students of the first year to wear a gown, he steadily refused, during this year, to exhibit himself in the accustomed academical habiliment.

This session proved a decisive one as to the course of his future studies. The Earl of Kinnoull, then Chancellor of the University, having been informed of his remarkable abilities, sent for his father, and proposed to him to defray the expense of his son's education, provided the father would agree to maintain him at college, with the view of qualifying him for the Church. The proposal was readily embraced; and the repugnance of the youth to apply himself to Latin was at length overcome, by making the permission to attend the Natural Philosophy class of next session,—the great object of his desire,—conditional on his agreeing to qualify himself, during the vacation, for attending also the Latin class. With this lure before him, he applied assiduously to his lessons, under the direction of a private teacher, and succeeded in fitting himself for admission into that class. No one could discover, in his after life, any traces of this early and vehement dislike to Latin; for though he ever held * that the learned languages were suffered to engross too much attention in our system of education, and was by no means sparing of reprehension upon this subject, his own scholarship had become considerable; and indeed his writings manifest a more than ordinary degree of fondness for embellishing the conclusions of science with illustrations from the Greek and Latin Classics. He continued to the last to read them occasionally, particularly Lucretius, whose bold and imaginative philosophy, and splendid descriptions, were peculiarly adapted to his taste. He came also to be well versed in the theory of grammar; and his observations upon languages often evinced learning as well as ingenuity.

His health, at the period above alluded to, was still so delicate that it became necessary to moderate and regulate his studious habits; but he succeeded, during his second session, in acquiring additional honours; and in attracting in a more marked degree the flattering attentions of the Chancellor, who kindly invited him to Dupplin Castle, where, notwithstanding the bashfulness of his manners, he contrived to impress the other visitors with a high opinion of his powers. About the same time, he became known to Mr Playfair, on occasion of a visit which the latter, alongst with the Reverend Dr Small, made to St Andrews. Dr Small's son was a fellow-student and companion of young Leslie, and hence his introduction to both these mathematicians. Mr Playfair was at this time parish Minister of Liff, in Forfarshire. Here he was afterwards visited by his young acquaintance, neither of them then dreaming of that lot which was to place both, in succession, in two conspicuous Chairs of the University of Edinburgh, destined to derive from their talents additional lustre and recommendations. His visits to Mr Playfair were continued after the latter, in 1782, resigned his clerical charge, in order to superintend the education of the present Robert Ferguson, Esq. of Raith, and his brother General Sir Ronald Ferguson. It was in this way, we believe, that he first became known to these excellent and distinguished men,

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* "The great error in modern education consists in the undue attention paid to the dead languages, which consumes the precious time that should be devoted, during the freshness of youth, to the higher intellectual pursuits." Preface to *Rudiments of Geometry*, published in 1828.

then youths of his own age; and the acquaintance, in after years, ripened into a warm and lasting friendship, alike honourable to both parties, and which formed one the chief and most valued solaces of his life.

In 1783 or 1784, he quitted St Andrews, and proceeded to Edinburgh, with the intention of entering himself as a student of Divinity in the Metropolitan University. He was accompanied, we believe, by another young Mathematician, destined, like himself, to obtain a distinguished niche in the Temple of Fame—James Ivory; and they lived together for some time. He never had any liking for the Church as a profession; and though he was formally entered at the Divinity Hall, he contrived to devote his first session to the sciences, particularly to Chemistry. In fact, he seems early to have relinquished all thoughts of the Church;—a resolution perhaps hastened by the death of his patron, the Earl of Kinnoull, which took place soon after his removal to Edinburgh. He continued to study here till the close of the session of 1787; and, as is customary with students of greater ability and industry than means, devoted part of his time to private tuition. One of the young men whose studies he assisted was nearly related to, and became the heir of Dr Adam Smith;—a circumstance which he was accustomed to recollect with pleasure, as having made him known to that illustrious Philosopher, who treated him kindly, and occasionally favoured him with directions as to his own pursuits. His first essay as an author must have been composed about the time of his leaving this university. It was a Paper *On the Resolution of indeterminate Problems*, which was read to the Royal Society of Edinburgh by Mr Playfair, in 1788, and afterwards published in its *Transactions*.*

In this year, he was prevailed upon by two young students of the name of Randolph, and of the distinguished American family of that name, to accompany them, in capacity of tutor, to Virginia; and he accordingly left Scotland alongst with them. They arrived at the place of their destination in the beginning of November; and his time afterwards seems to have passed both agreeably and usefully. He was all his life fond of visiting other countries, and perhaps a little disposed to underrate his own. Thus he was wont to say, that the thunderstorms of Virginia took away all feelings of awe at those of Scotland, just as the Alps of Switzerland left him nothing to admire in the Scottish mountains. His stay in the New World did not much exceed a year, owing to the breaking up of the family establishment by the death of the father of his young friends. After visiting New York and Philadelphia, he returned to his native place towards the close of the year 1789.

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* *See vol. ii. p. 193.*

From some letters to his family, written about the time of his leaving America, his thoughts seem to have been anxiously directed to his future means of employment and support; and one of his schemes appears to have been to try his fortune in India, probably as a Civil Engineer. This notion recurred afterwards, but without leading to any results. His next field of adventure was London, whither he proceeded in January 1790, carrying with him various letters of recommendation; one of which was written by Dr Adam Smith, then drawing near the close of his memorable career. It was on this occasion, if our recollection does not mislead us, that Dr Smith exhorted his young friend never to approach any author whose favour he might wish to win, without first reading his book, lest the conversation should happen to turn that way. One of Mr Leslie's objects in visiting the capital, was to ascertain what success he might expect from a course of Lectures of Natural Philosophy; and the information he received soon satisfied him, as he says in a letter to one of his brothers, that "rational lectures would not succeed." He therefore employed himself for some time in writing for the *Monthly Review*, and in executing literary jobs delegated to him by his countryman Dr William Thomson, author of the continuation of Watson's *History of Philip the Third*, and of many other works now forgotten; and who was much in the habit of lending his versatile pen, as well as his name, to those who required the assistance or recommendation of either. But a more eligible and suitable connection was ere long opened to him, by the invitation of the younger Wedgwoods, who had been his fellow-students at Edinburgh, to reside with them, and superintend their studies. He readily acceded to their proposals; and proceeded, in April 1790, to their residence at Etruria, in Staffordshire, where he remained till the close of 1792, in the enjoyment of a liberal salary, and of society at once agreeable and intelligent. This was, in every sense, a happy and prosperous period of his life. The time not devoted to tuition was assiduously employed in experimental investigation, and in completing a translation of Buffon's *Natural History of Birds*, which he had previously undertaken for a London bookseller. It was published in 1793, after he left Etruria, in nine volumes octavo. Though executed with fidelity and vigour, it was valued by himself, at least after he became otherwise eminent, only as having, by the sum which it procured him, laid the foundation of that pecuniary competency which he early foresaw to be necessary to the independent prosecution of his favourite studies; and which his industrious and prudent habits enabled him in no long time, in a moderate degree, to attain. The preface, however, alludes to this "first attempt" with considerable anxiety; and endeavours to bespeak favour for it as executed "at an early period of life, and in the retirement of the country." This preface is written in that nervous, but strained and ornate style, which characterizes all his after writings. His first contribution to Natural Philosophy, entitled *Observations on Electrical Theories*, was also written at Etruria; and was, in 1791, transmitted to the Royal Society of Edinburgh, for insertion in its *Transactions*. This was promised; but performance having, as he thought, been long and unhandsomely deferred, he indignantly recalled his paper, and laid it aside for many years. It did not, indeed, see the light till the year 1824, when he was induced to publish it in the *Edinburgh Philosophical Journal*,* conducted by his distinguished friend Professor Jameson.

About the period of the close of his agreeable engagement at Etruria, which was occasioned, we believe, by the ill health of Mr Thomas Wedgwood, and the marriage of his younger brother, a Cornish gentleman with whom he had become acquainted at that place, proposed to engage him as a companion in an extensive tour through Greece, Egypt, and the Holy Land. Ever fond, as he was, of visiting foreign countries, particularly those hallowed by memorable events or classical associations, the proposal could not but prove agreeable to him, and it was accordingly embraced with alacrity. The tour was to commence early in 1793; but a change of plan having been decided upon without his concurrence, and by which he found that his observations were to be transferred to another, he immediately relinquished his engagement. His intention to visit these interesting countries, when a fit opportunity should occur, remained with him through life; and, at a period nearly forty years subsequent to this,—in his last years, indeed,—he actually made some preparations for a year's sojourn in Egypt and Palestine, from which he was diverted only by engagements and avocations of which he found it difficult to disencumber himself.

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* *See vol.* xi. p i. of this Journal.

After leaving Etruria, he passed some months in Holland; a country which, like Descartes, he seems to have thought peculiarly suited to secluded study, and where he at this time acquired the German language. Thereafter, he returned to Largo, where he remained for about two years, devoted to experimental researches; in the course of which he invented and perfected his Differential Thermometer; the parent, if we may so speak, of that beautiful family of Philosophical Instruments,* with which he enriched the Treasury of Science, and amplified and variegated the means of physical inquiry. His ingenuity had been early exercised in some attempts to construct an accurate Hygrometer, and these ultimately suggested to him the well-known contrivance above named;—a contrivance happily adapted to the measurement of the smallest variations of temperature, and which richly rewarded his inventive powers by its ministry to the achievement of his subsequent discoveries. It has generally, indeed, been allowed to be one of the most useful as well as elegant inventions that inductive genius ever applied to the investigation of chemical changes. At a later period, when his name had attained a high degree of celebrity, he, like most of the other sons of genius, was made to feel that fame brings with it pains as well as pleasures; for it was now rumoured that the Differential Thermometer, instead of being an invention of his own,—perfected, as he has himself recorded, in the course of a series of experiments on the evaporation of ice, which the severe winter of 1794–5 afforded him an opportunity of performing,*—was in reality a plagiarism, if not from Van Helmont, who died in 1644, at any rate, from John Christopher Sturmius, who died some sixty years later. Such was one attempt to impeach the originality of this eminently inventive experimentalist. An instrument, of which all the world remained ignorant, till he, by means of it, told the world the better part of all it yet knows concerning the phenomena of radiated heat, was discovered to have been furtively purloined from one or other of these antiquated worthies! Neither the authors nor the abettors of this allegation pretended that either Van Helmont or Sturmius ever dreamt of such applications, or derived such results from their supposed invention, as were reserved, by some caprice of chance, no doubt! for its luckier plagiarist; and it is, we believe, generally admitted, that this is just one of those cases of curious but partial anticipation so frequently to be met with in the history of science; and where the ultimate inventor shews, by his skilful and fruitful employment of the disputed invention, how much he surpassed, and how little he needed the help of those whom he is ungenerously supposed to have robbed of their legitimate honours.

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* These instruments, viz. the Differential Thermometer, Hygrometer, Hygroscope, Photometer, Pyroscope, Æthrioscope, and Atmometer, are all described, and their principles explained, by himself, in the articles Climate and Meteorology in this Encyclopædia. Properly speaking, the Hygrometer was the *parent* instrument; the Differential Thermometer having been invented in the course of his endeavours to improve it; but as the Hygrometer, in its latest m, is only a modification of the other, it may be represented as derived from it.

In the spring of 1796, Mr Leslie received an invitation from his friend Mr Thomas Wedgwood, to accompany him in a tour through the north of Germany and Switzerland. To this proposal, which was every way agreeable, he immediately acceded. They arrived at Hamburg about the first of May, and employed that and the next four months in their tour, part of which was performed on foot. He alludes to some observations which they made in the course of their journeyings amongst the mountains of Switzerland, in one of the notes to his celebrated work on Heat; but of this, as of all his subsequent tours on the Continent, he kept regular Journals, which are still preserved; and which shew that he was no less observant of the social, moral, and economical condition of the countries he visited, than of their geological, meteorological, and physical aspects.

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* See the article Meteorology in the *Encyclopædia*.

For some years after the period just mentioned, he seems to have employed himself in experimental pursuits, and to have divided his time, chiefly, between London and Largo; to which place, and to the society of his family, he ever was fondly attached. His earnings by literary labour, and his allowances, had raised his humble fortune to what a Philosopher might view as an independence; and he accordingly employed or amused himself as his own inclinations dictated. Early in the summer of 1799 he set out with an old college acquaintance on a tour to the northern kingdoms of Europe; in the capitals of which, the latter, who had for some time been settled in Spain as a merchant, had business to transact. After traversing Denmark, part of Norway, and Sweden, they proceeded through Brandenburg to Berlin; and from thence returned to England about the end of November. From his journal of this tour, which is more detailed than any of those he subsequently kept, the Swedish mines appear to have formed particular objects of his attention. One of his entries records his having, before quitting Hamburg, written an account of his Hygrometer, for insertion in Voght's *Magazine*, and in the *Annales de Chimie.*

In the following year, he published the paper just mentioned, but with some alterations, in Nicholson's *Philosophical Journal.* It is entitled a *Description of an Hygrometer and Photometer;* and was followed with *Additional Remarks* on these instruments. In the same year, he also published, in that Journal, a Paper *On the Absorbent Powers of the Different Earths;* and other two, containing *Observations and Experiments on Light and Heat, with Remarks on the Inquiries of Dr Herschel on these objects.** These small pieces are very valuable, as shewing the progress of his researches and discoveries in that field of inquiry which their titles indicate.

The results of his more extended investigations were, ere long, to appear before the world in a different shape. Having collected at Largo all the necessary apparatus, he prosecuted with ardour a series of experiments, which enabled him, in the years 1801 and 1802, to compose the bulk of his celebrated work on Heat. In the latter year, "the gleam of peace," as he tells us in his usual ornate style, "tempted him to indulge in a temporary suspension; and to repair to the famed capital where the treasures of art and science are so profusely displayed. In that vortex of pleasure and centre of information, I spent," he adds, "several months very agreeably; but the work I had undertaken recalled my thoughts, and I hastened again to my retreat."* His *Experimental Inquiry into the Nature and Properties of Heat* was here at length completed. It was published at London in the spring of 1804, in an octavo volume, with a dedication, couched in terms of strong and affectionate friendship, to Mr Thomas Wedgwood, the companion of his studies at Etruria, and of his first continental tour. The early death of that ingenious and excellent person, whose delicate health is here feelingly alluded to, was always mentioned by him as a public as well as private misfortune. The originality and boldness of the peculiar doctrines of the *Inquiry*, and the number of new and important facts disclosed by its singularly ingenious experimental combinations, conspired to render it an object of extraordinary interest throughout the scientific world; and, indeed, it must ever be viewed as constituting an era in the history of that recondite branch of Chemical science which forms its subject. The Council of the Royal Society of London unanimously adjudged to its author the Rumford Medals appropriated as the reward of discoveries regarding the nature of heat. As a philosophical disquisition, it is far, however, from being perfect. Its hypotheses are not warranted by the sober maxims of inductive logic; and its method and style are alike liable to serious criticism. But it would be difficult to name any work in the whole range of modern science more strongly indicative of a vigorous and inventive genius; and it must be allowed by all, that its skilful experiments, and its large stock of new observations, far more than atone for its questionable theories, and for that desultory arrangement, and those ambitious modes of expression, which so often mar its reasonings and obscure its sense. More than ten years before the appearance of this work, Mr Leslie wrote an essay on *Heat and Climate*, which contains some of its theoretical opinions, as well as the germs of some of its discoveries. It was read at two successive meetings of the Royal Society of London in the spring of 1793; but it was not admitted into the *Transactions* of that body. Its author was not of a disposition to be checked in the career of inquiry by this repulse; and he did not shrink from bringing his paper forth, in 1819, from its long oblivion of twenty-six years; he having then published it, for the first time, in Dr Thomson's *Annals of Philosophy*.*

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* See Nicholson's Journal for 1800, vol, iii. p. 461–518, and vol. iv. p. 196, 416.

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* Preface to the Inquiry on Heat.

Twice before that period of his life at which we are now arrived, Mr Leslie had appeared as a candidate for an Academical Chair;—first in the University of St Andrews, afterwards in that of Glasgow, and on both occasions without success. He was again to try his fortune in the same line in the Metropolitan University. Early in the year 1805, a vacancy occurred in its Mathematical Chair, owing to the removal of Professor Playfair, on the death of Dr John Robison, to that of Natural Philosophy. This afforded a new opening to his academical ambition; and he was particularly desirous to occupy a Chair upon which the names of the Gregories, of Maclaurin, of Matthew Stewart, and of Playfair, had shed so much lustre. He accordingly presented himself as a candidate; and, with his now high reputation as a discoverer and original thinker, and his acknowledged eminence in mathematical science, it was not to be expected that he could have any formidable competition to surmount. Nor did there occur any, in as far as fame or talents were concerned. His principal competitor, though a man of amiable character, and respectable attainments, was wholly unknown in the scientific world, and as inferior to him in abilities as in renown. But he was one of the Ministers of the City, and supported with all the influence of that body; then pretty generally suspected of a wish to secure a monopoly of the Philosophical Chairs of the University. It soon, however, became known, that the Patrons were determined to de-

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See vol. xiv. p. 5–27, and vol. xvi. p. 7, for some just observations by ble Editor.

cide upon a comparison of claims, and that Mr Leslie must triumph. In this state of things, and in an evil hour for themselves, the City Clergy were induced to raise an objection to his eligibility, on the serious ground of his having, in one of the notes appended to his work on Heat, approved of a doctrine directly leading to atheism. We are sorry to be obliged to notice this discreditable proceeding; but it forms too memorable an occurrence in Mr Leslie's life to allow us to pass it without some animadversion. In the note alluded to, the author, though no Metaphysician, and in general rather a contemner of metaphysical science, was, naturally enough, led to illustrate what he had said in the text, with reference to the unphilosophical opinion that impulse is necessary to the production of motion, by some remarks on *Causation*. He prefaces these remarks by observing, that Mr Hume was the first who treated this subject " in a truly philosophical manner;" and that " the unsophisticated notions of mankind are in perfect unison with the deductions of logic, and imply nothing more at bottom, in the relation of cause and effect, than a constant and invariable sequence."* Founding upon these observations, the Ministers of Edinburgh charged him with having " laid a foundation for rejecting all the argument that is derived from the works of God to prove either his Being or Attributes."† This heavy charge was preferred in a formal Protest, tendered by them to the Patrons of the University; in which they alleged that, in the election of Professors, the former were, by the Charter of erection, bound to act *with the advice of the Ministers*. That advice was, in the present instance, given with sufficient emphasis; but the Patrons, much to their honour, treated it as it deserved; and Mr Leslie, to the great joy of all liberal minds, was, in March 1805, elected to the Mathematical Chair. The efforts of the disappointed junto did not, however, cease with this rebuff; nor did they desist from their ill-starred opposition, till a decision of the General Assembly of the National Church, pronounced on the 23d of

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* *Inquiry into the Nature of Heat*, p. 135, and Note 16, p. 522.

† See Professor Stewart's *Short Statement of Facts relative to the Election of a Mathematical Professor in the University of Edinburgh*, p. 44.

May, after a memorable debate of two days,[*] satisfied them that persecution had now exhausted its resources, and that its hopes must, however sorrowfully, be relinquished.

Dismissing every supposition of interested designs, and even allowing that Mr Leslie's expressed opinion as to *Causation*, if taken apart from the subject-matter of his book, or left unexplained, was calculated to occasion some alarm in the minds of the pious, still, impartial history ever must brand the proceedings of his opponents as alike uncharitable, unfair, and arrogant; and as deeply injurious to the character of that Church with whose name and authority they clothed themselves. It was on all hands admitted, that if Mr Leslie had, by a single word, limited his observations to *Physical* causes, his doctrine would have been wholly free from objection; and, surely, it required a most perverse and intolerant construction to insist on extending to any but such causes, the observations of an illustrative note to a work purely physical, and which were obviously levelled at those theories which resort to certain invisible *intermedia*, in order to account for the connection of physical sequences. But this was not all. Mr Leslie, on being informed of the charge, immediately declared, in a very pointed Letter laid before the junto, that his observations "*referred entirely* to the relation between cause and effect, considered as an object of *physical* examination."[†] Yet was this prompt explanation disregarded—nay suppressed; whilst his persecutors—owing to an ignorant blunder in their own statement of what they conceived to be the true notion of *Causation*—were themselves obliged to have recourse to explanation, in order to show that their doctrine was not identical with that of the Fatalists and Spinoza![‡]

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[*] See *Report of the Debate*, published at Edinburgh in October 1805.

[†] See Professor Stewart's *Short Statement*, p. 36, and *Report of the Debate in the General Assembly*, p. 16.

[‡] See *Short Statement*, p. 77–94, and *Report of the Debate*, *passim*. This remarkable controversy gave rise to a number of other publications; but none of them, with the exception of these two, an admirable *Letter* to the author of a Reply to the former, by Professor Playfair, and Dr Thomas Brown's *Observations on the nature and tendency of the doctrine of Mr Hume concerning cause and effect*, with other two pieces by that most acute metaphysician, have

Mr Leslie commenced and prosecuted his official duties with great ardour. He entertained lofty ideas of the dignity and utility of the professorial character, and was thus disposed to make all the exertions necessary to success. Though the bent of his genius lay more to Physics than to Pure Mathematics, he had cultivated the study of Geometry with kindred relish; and with an admiration, in particular, of the analytical investigations of the ancient geometers, which led to his happiest essays in that science. As a teacher, he not only laboured to promote the study, but to procure for it a larger share of attention than our academical system usually assigns to it. His instructions were better suited, perhaps, to youths of superior ability than to ordinary students; but reputation, and intellectual power, produced their usual results, and secured for him an attendance as numerous as could be expected by any teacher, during the whole of the fifteen years that he occupied the Mathematical Chair. Soon after his election, he resolved to compose and to publish, at successive intervals, a complete "Course of Mathematics," digested and arranged according to his own ideas of what was wanted towards promoting a purer taste in the cultivation of the science. Of this Course, the first volume, comprising *Elements of Geometry, Geometrical Analysis, and Plane Trigonometry*, was published in 1809; and it has gone through several extensive editions. The first part of it has been regarded as the least perfect and useful; but his own favourite portion, on *Geometrical Analysis*, has been extolled even by the most unsparing critics of the former, as "a great acquisition to Elementary Geometry; and as calculated to keep alive the knowledge of a most beautiful and interesting branch of the mathematics, which has been too much overlooked during the improvement of the more general and powerful methods of algebraic investigation."† Abroad, it seems to have been viewed in a light equally favourable; as it was speedily translated into

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the least chance of interesting posterity. The two pieces alluded to were, *A short criticism of the terms of the charge against Mr Leslie in the Statement of the Ministers of Edinburgh*, and *An examination of some remarks in Dr Inglis's Reply to Professor Playfair*. The Reply by Dr Inglis was the ablest production on the clerical side, and certainly evinced a good deal of controversial ability.

† *Edinburgh Review*, vol. xx. p. 98.

the French and German languages. He reproduced it, with considerable emendations, in the second volume of his "Mathematical Course," which, besides, contains the *Geometry of Curve Lines.* This volume was not published till 1821—an interval of twelve years having thus elapsed from the appearance of the first. A third volume, on *Descriptive Geometry, and the Theory of Solids*, was still wanting to complete his original design; but his removal to another Chair, and other circumstances, called his attention to different objects, and his "Mathematical Course" was thus left unfinished. He was induced, at a late period of his life, to recast the first volume in a greatly abridged form. His object in doing so was to accommodate it to the use of those who, in riper years, became desirous to supply the defects of early education, and to qualify themselves for obtaining some knowledge of Natural Philosophy. This abridgment appeared in 1828, under the title of *Rudiments of Plane Geometry, including Geometrical Analysis, and Plane Trigonometry.* In connection with his mathematical works, though forming no part of his "Course," we may here mention the profound and learned treatise on the *Philosophy of Arithmetic*, which he published in 1817. It was a republication, with considerable alterations and additions, of one of the numerous articles contributed by him to the *Supplement* to the former editions of this Encyclopædia.

It was not to be expected that the labours of the Mathematical Chair would wean Professor Leslie from his favourite experimental inquiries. His fine instruments were always at hand, and always in use, in connection with some ingenious conception or other. Early in the summer of 1810, he determined to proceed with a set of experiments previously suggested in the course of his researches with his Hygrometer, but which had for some time been suspended; and they now, on being resumed, conducted him to the discovery of that beautiful process of Artificial Congelation, by which he was enabled to produce ice, and even to freeze mercury, at pleasure. The discovery was achieved by means of a happily conceived combination of the powers of rarefaction and absorption, effected by placing a very strong absorbent under the receiver of an air-pump. It was in the month of June of that season that the discovery was consummated.* We happened to witness this consummation,—at least the performance of the first successful repetition of the process,—and we never shall forget the joy and elation which beamed on the face of the discoverer, as, with his characteristic good nature, he patiently explained its principles, and the steps by which he had been led to it. We could not but feel, on looking at, and listening to him, how noble and elevating must be the satisfaction derived from thus acquiring a mastery over the powers of Nature, and enabling man, weak and finite as he is, to reproduce at pleasure her wondrous works! Proportioned to the admiration which such achievements are calculated to excite, ought to be the disapprobation of any unfair endeavours to lower or depreciate them. We have already alluded to an attempt to divest this illustrious experimentalist of the honours connected with his Differential Thermometer; and we have now to add, that several years after the discovery of his process of Artificial Congelation, some similar endeavours were exhibited to transfer the merit of that discovery to a person of the name of Nairne. The claim for him was founded on a Paper published in 1777, in the *Transactions* of the Royal Society of London; from which it appears that he was acquainted with the facts, that evaporation produces cold, and that sulphuric acid, the absorbent employed by Mr Leslie, imbibes moisture. But, in order to decorate Mr Nairne with the laurels of the latter, his depreciators ought to have been able to shew, that the former had actually combined the properties alluded to, in a manageable process for the production of ice *ad libitum*. In such an attempt they must have failed ignominiously; and, perhaps, there is not in the whole history of science any more triumphant reply to a charge of plagiarism, than is furnished by the admitted facts, that with Mr Nairne's Paper before it for a long course of years, the scientific world remained utterly ignorant of the existence of any such process till the date of Mr Leslie's discovery;—nay, that with his description of that process in their hands, the most distinguished experimentalists of the capital failed in their trials of it, till it was performed there by himself, in the ensuing summer ![*]

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* See the article COLD in the *Encyclopædia Britannica*. The successive steps of the discovery are here recited by himself, in those *verba ardentia* which the bent of his genius so strongly prompted him to employ.

In a letter from London to one of his friends, written in June 1811, he says,—"My package has at last arrived, and I shall proceed without delay to make my *debut*." It was only now that the experiment was first successfully performed in the capital. This took place in presence of several members of the Royal Society. The discovery was announced in the same year, in the Memoirs of the French Institute; and the process itself was afterwards exhibited before that body, by M. Pictet and M. Gay-Lussac.[†] He did not himself publish any detailed account of his experiments till 1813, when he explained them at considerable length, in a small volume entitled, *A short Account of Experiments and Instruments depending on the Relations of Air to Heat and Moisture.* This publication, which was partly intended to promote the circulation of his Instruments, and to explain their principles, would have been infinitely more useful had its author superadded the powers of methodical and elementary exposition to his other high endowments. But, notwithstanding its defects, in these particulars, it was much commended by those competent to appreciate its value as a contribution to science.* Closely connected with the subject of this treatise, and which we may therefore notice here, though it did not appear till some years later, was an ingenious Paper, published in 1818, in the *Transactions* of the Royal Society of Edinburgh, under this title: *On certain impressions of cold transmitted from the higher atmosphere; with a description of an Instrument adapted to measure them.* The Æthrioscope, the instrument here alluded to, is, in another place, described, in the poetical language of its author, as "fitted to extend its sensation through indefinite space, and to reveal the condition of the remotest atmosphere."

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[*] Some equally complete and curious legal evidence of these facts was adduced by Professor Leslie, in 1822, in a prosecution which he was advised to institute against the publisher of a well-known *Magazine*, for a series of libels inserted in that work; in one of which he was accused of having stolen the discovery alluded to from Mr Nairne. Amongst other witnesses, two very distinguished Chemists were examined on that occasion—the late Dr Marcet, and Dr Thomson, Professor of Chemistry in the University of Glasgow. The evidence of both was equally favourable to Professor Leslie. We shall extract a small portion of that of Dr Marcet, to whom the Professor was personally but little known:—"*Q.* Is it your opinion that Mr Leslie is to be considered as having borrowed or stolen this discovery, or do you consider his discovery to be original?—*A.* Some of the facts were known long before, but the process itself is perfectly original.—*Q.* Is the discovery of Mr Leslie analogous to other discoveries in the science of chemistry?—*A.* There is hardly any discovery of the least value that has been made in that science but from the known properties of bodies. It is by combining those properties, so as to produce certain effects, that a discovery is made.—*Q.* Then you mean to say that Mr Leslie has done what none before him ever accomplished?—*A.* Certainly. *He has done what the whole philosophic world, with all the facts before them for a long period, had not been able to accomplish.*—*Q.* When and by whom was the experiment first successfully performed in London?—*A.* It was successfully performed in London by Mr Leslie himself. My belief is, that no one succeeded in this experiment, in London, until Mr Leslie himself shewed the way.—*Q.* Do you know that Sir Humphrey Davy tried and failed?—I cannot positively say; I believe he tried it, but without success."—(*Report of the Trial by Jury, Professor Leslie against William Blackwood*, p. 82–6.)

[†] See *Mémoires de la Classe des Sciences Mathématiques et Physiques*, t. xii. p. 80; t. xiv. p. 117–18.

In the autumn of 1814, Professor Leslie indulged himself with a tour of six weeks in France and the Netherlands. He never was satisfied if he allowed a vacation to pass without visiting some foreign scenes. One or two extracts from letters written by him on the present occasion, may be here introduced, as either curious in themselves, or characteristic of the writer. Writing from Paris, on the 1st of August, he says, "You know that it was not my intention at present to mix much with the *Sçavans.* But I have been so well received, and even feasted by them, that I may perhaps depart a little from my original design. Humboldt has been very kind and attentive to me, and introduces me wherever I want. They are much better acquainted with what we are doing than I should have imagined. My book on Heat is better known than in England. I was even reminded of some passages in it which in England were considered as fanciful, but which the recent discoveries on the Polarity of Light have confirmed. Even Laplace has, in consequence of some observations of mine, silently omitted a passage in the last edition of his *Système du Monde.* I paid a visit the other day at Arcueil. Berthollet has a fine chateau seated on a bank amidst gardens, vineyards, &c.; and Laplace has another, little inferior, and adjoining to the grounds. I dine with Laplace next Sunday. Some person had informed him that I was the author of a critique in the *Edinburgh Review* on a paper of his, and he had sent an answer to me, which, however, I never received."* The following extract from another letter, written at Bordeaux, in the beginning of September, gives a rapid and lively sketch of his journey to that place:—" My tour has furnished what I wanted—a number of images of the milder and hotter regions of the Continent. From Paris I proceeded to Macon, over a rich and well cultivated country, covered with wheat and vineyards, the crop for the most part already gathered in. Thence I descended the Somme, a fine, clear river, to Lyons; the banks covered with luxuriant vineyards stretching to a range of hills, and the waved surface sprinkled with trees, intermixed with frequent villages, and lively villas, all of white limestone. At Lyons I met with the celebrated Baron Zach, and was conducted by him in his carriage to the fountain of Vaucluse, Avignon, and thence to Marseilles. His society was particularly entertaining and instructive. We now passed through the country of the mulberry, the fig, and the olive; but I confess that Provence did not come up to my expectation. I have seen, what I had much longed to see, the awful fountain of Vaucluse, and the blue expanse of the Mediterranean; but the shores at Marseilles are terminated by gray, naked rocks, shooting fantastically to great heights. I staid some days at Marseilles, and spent most of my time at a country house occupied by the old Duchess of Saxe-Gotha, with whom Baron Zach lives in quality of Chamberlain. I found her very kind and affable, and extremely glad to hear any news of the Royal Family of England. From Marseilles I returned to Avignon, and then crossing the Rhine, proceeded to Nismes, over a country extremely fertile and interesting. This is almost a Roman town. There is a temple to Augustus, beautifully Corinthian, a fine temple of Diana, and an amphitheatre almost entire, capable of holding twenty thousand spectators. I now proceeded to Montpellier, and saw the majestic range of the Pyrenees stretching on our left, and covered with eternal snow. Montpellier is an interesting place,—its Botanic Garden rich, and its promenades superb. I then proceeded to Toulouse. In leaving it, the carriage plunged into a hollow across the road; it was eleven at night, but the moon shone full, and lighted up a fine rich plain. I shuddered when told that we had just crossed 500 dead bodies, which had been thrust into a cut or trench of the road, after the late battle. The road to Bordeaux runs near the course of the Garonne, through one of the finest and richest countries I have ever seen." From this place he returned to Paris, and, after a short stay, proceeded through the Netherlands to Rotterdam, where he took shipping for Scotland.

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* See *Edinburgh Review*, vol. xxiv. p. 339–52; and Dr Thomson's *Annals of Philosophy*, vol. ii. p. 457–62, for a skilful analysis of the treatise. In the trial above alluded to, the party prosecuted took an *Issue* to shew, that the above article in the *Edinburgh Review*, in commendation of the work, was written by Mr Leslie himself! The attempt was not made; and it is hardly worth while to mention, with reference to such a charge, that the article was written by that very able chemist, the late Dr John Murray.

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* This must allude to a paper by Laplace *On the Motion of Light in Diaphanous Media*, published in the *Mémoires de la Societé d'Arcueil.* These Memoirs were reviewed by Professor Leslie, in two articles, in the fifteenth volume of the *Edinburgh Review.* They formed his first contributions to that Journal.

The publication of the *Supplement* to the three preceding editions of this Encyclopædia commenced towards the close of the year 1815, and was continued progressively till its completion early in 1824. To that work, which was undertaken upon a very extensive plan, and which aimed at procuring the highest attainable assistance, Mr Leslie was throughout a contributor. His contributions, surprisingly numerous when his other avocations are considered, display all the powers and attainments for which he was remarkable. Nor was it by his writings alone that he aided this publication. His advice, his invaluable information,—amazing alike for its minuteness and extent,—and his influence, were always at the service of its Editor, whose acknowledgment of these various obligations has long been before the public.* But it is due to Mr Leslie's memory to specify in this sketch of his life what the work owes to his genius and knowledge. Ranged in alphabetical order, his writings in it occur under the following heads: Achromatic Glasses; Acoustics; Aeronautics; Andes; Angle; Angle, trisection of;

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* See Preface to the Supplement, p. 13.

Arithmetic, palpable and figurate; Atmometer; Barometer; Barometrical Measurements; Climate; Cold and Congelation; Dew; Interpolation; Meteorology.*

Another work which at intervals enjoyed the benefit of his co-operation, was the *Edinburgh Review*. But his contributions, though commencing in the year 1809, were not numerous. They helped, however, and that in an eminent degree, to strengthen and diversify the Scientific department of that Journal; whilst those respecting Voyages and Travels combined scientific observation with powers of writing of no ordinary description. Among his principal articles may be mentioned those on the Physical and Chemical Memoirs of the Society of Arcueil; on the history of the Barometer; on Delambre's Arithmetic of the Greeks; on Von Buch's Travels; on Humboldt's Physical View of the Equatorial Regions, and Travels; and on the attempts to discover a North-West Passage to Asia. The picture in the last, of the revolving year, as observed within the arctic circle, is executed with a force of conception, as well as of colouring, that have not often been surpassed.

In the year 1819, a new field of professorial labour was opened to Mr Leslie, by the vacancy in the Chair of Natural Philosophy, occasioned by the death of one of the greatest ornaments of the University, Mr Playfair. Mr Leslie was on his return from one of his summer trips to the Continent when this lamented occurrence took place. The news met him on being put ashore at Largo, and is thus mentioned in a letter from that place, written on the first of August; in which he also describes an accident that had nearly, as he says, deprived the University of another Professor: "After having been detained for about a week in Holland, and after a tedious but agreeable passage of near ten days, I was at last put ashore here, from a sloop bound from Rotterdam to Grangemouth. Every thing looked joyous; but I had soon the tidings of poor

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* The present edition is enriched with the whole of these articles. That part of the article *Arithmetic*, which relates to palpable notation, has been omitted, with the exception of the curious disquisition on the *Abacus*, which is printed separately in this edition. The article *Interpolation* is, in it, annexed to that on Logarithms.

Playfair's death, which was most unexpected and distressing. The loss to the University is severe.........I suspect that you were nearly deprived of two professors at once; for on Tuesday the 13th ultimo, I met with an alarming accident in Holland. I had passed the evening at a clergyman's with an intelligent merchant, late Provost of Aberdeen. On coming out of the house, at the end of the Boomtiges Street, or the street with rows of trees, the Maas running close, I stepped hastily forward to show him the Comet, and the quay being quite dark, I fell eight feet, and then plunged four feet in the water. This was the affair of an instant, and I felt that I was drowning; but I quickly recovered myself, and was helped out, with no damage but that of a bruize sustained in the fall." The eyes of the Patrons, and others interested in the University, were now turned to Mr Leslie, as the person best qualified to fill the vacant chair. Three years before, when Mr Playfair was abroad, and an arrangement failed for carrying on the Class, Mr Leslie, at the very commencement of the session, unhesitatingly undertook the task for his absent colleague;* but, independently of this circumstance, his eminence in mathematical and physical science was such, that no one else could reasonably be thought of; and he accordingly was, without difficulty, appointed to the Chair—being thus a second time, but in more melancholy circumstances than on the former occasion, nominated a successor to his early friend.

The Chair to which he was thus unexpectedly called was unquestionably that for which he was best suited; and had he happened to be placed in it at the commencement of his professorial career, Science in all probability would have derived greater benefits than she actually reaped from his powers. The time spent in his mathematical compositions would have been more profitably employed in that wider and richer province which he was so peculiarly qualified to cultivate. One of the first cares of his new situation; was, the extension of the apparatus required for that greatly enlarged series of experiments which he thought necessary for the illustration of the Course. This, indeed, was an object of which, from the first to the last year of his incumbency, he never lost sight; and it is due to him to state, that it was through his exertions that the means of experimental illustration, in the Natural Philosophy Class, were for the first time made worthy of the University. Viewing him merely as a lecturer, it must be admitted that he was not eminent. He was apt to forget, or rather did not perceive, that the connecting links between premises and conclusion, though familiar to the teacher, may, to the learner, be all unknown; that views quickly reached by the acquired perceptions of the one, must be opened up, step by step, to the yet imperfect vision of the other; and that it is the imperative duty of a public instructor to bring down knowledge from its highest spheres, and place it on a level adapted to the powers of unpractised understandings. His views of the nature of science were grand and animating; and his strictures on the great discoveries which constitute the epochs of its history, sometimes swelled into lofty strains of eloquence; but, generally, in lecturing, as in writing, he wanted that consecution of thought, and that perspicuity of exposition, without which reasoning cannot be made intelligible, nor its conclusions satisfactory. Still, the attraction of his numerous experiments, the celebrity of his name, and the opinion entertained of his extraordinary powers, joined with great simplicity and affability of manner, concurred to secure him the respectful homage of his students, and to sustain the glory of the University. In 1823, he published, chiefly for the use of his class, a volume entitled *Elements of Natural Philosophy*; being the first of a Course intended to extend to three, and to exhibit a comprehensive view of the principles of that congeries of sciences which we are accustomed to class under the above term. Here, as was the case with his Mathematical Course, his plan was not completed; for he published no part of it but the volume mentioned, which includes only Mechanics and Hydrostatics. A second edition of this volume, rrected, and augmented with Notes, was published in 1829, ree years before his death.

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* Mr Playfair expressed his gratitude, in very warm terms, for the promptness with which Mr Leslie, on this occasion, relieved him from what would have otherwise proved an unpleasant predicament; and it is but justice to the latter to mention the act, as showing that the placability of his nature, had entirely obliterated those feelings which the somewhat severe critique of his *Geometry* in the *Edinburgh Review*, known to have been written by Mr Playfair, would have left to rankle, and produce the ordinary results, in a mind more irritable or less magnanimous.

Mr Leslie had early determined to visit Italy at a fit opportunity; and he at last, in the summer of 1823, carried his design into execution. He set out with a mind still glowing with youthful enthusiasm, at the prospect of beholding the "eternal city;" but, whether from his entertaining too lofty conceptions, or from his view being too hurried and superficial, the tour, in as far at least as respected that crowning object of it, ended in disappointment. The following letter, written at Innsbruck on the 11th of August, contains a brief outline of his journey: "I have thus reached the frontiers of Germany on my return from Italy. I have fortunately achieved the principal objects of my tour; and though I have travelled slowly, I have seen a great deal in a short time. From Geneva I proceeded by Lausanne up the Valais to Brieg—thence crossed the Simplon, and descended through the plains of Lombardy to Milan. From this place I advanced by Parma and Modena to Bologna—next crossed the Apennines to Florence—again crossed another part of that broad chain to Rome. For various reasons, I made the "eternal city" the limit of my journey. I therefore traversed the Apennines again to Ancona—skirted a considerable portion of the Adriatic—returned to Bologna, and thence proceeded to Mantua. I spent two days, about twenty miles from that place, at an old chateau, the residence of Acerbi, who accompanied me to Verona. Thence I went to Trent, and journeying through the Tyrol, have reached this spacious, interesting city. Italy has rather fallen below my expectations, whilst Switzerland has surpassed my early impressions. The passage of the Simplon alone is worth the journey; but the rout through the Tyrol, though not so sublime, is highly picturesque. Italy has every thing on a grand scale. The plains, mountains, rivers, works of art, are all majestic and noble. But there is no comfort. Rome itself stands in the midst of a desert. Its grandeur has always been artificial—the result of force or fraud. I have seen whatever is most interesting; and I am inclined to differ very widely from our ordinary travellers. I heard much of the *malaria*, but I escaped untouched. I suffered little from the heat, and bore it better than the natives. Conformably to the custom, however, I was a sort of prisoner during a great part of the day—the windows shut to exclude every ray of light; and I was only called at times to look out, by the babbling chant of the Monks, with their torches and crosses, carrying the dead to their graves, and followed by the Charitable Brethren, like ghosts, apparelled in white sheets, with only holes for their eyes. They seemed better fitted to terrify the living than to comfort the dying." Two years after this, he made another tour on the continent, in which he seems only to have gone over ground in France, the Netherlands, and Holland, which for the most part he had traversed before. This, we believe, was the last of his journeys abroad.

The only important production of Mr Leslie's latter years was that which formed his crowning benefaction to this Encyclopædia —his Discourse on the *Progress of Mathematical and Physical Science during the eighteenth century*; which, with others of a similar description, constitute its first volume. The opening tribute to Mr Playfair, of whose history of the earlier progress of these sciences, this Discourse is a continuation, does honour alike to the writer's candour and taste. "The progress of mathematical and physical science during the brilliant period which closed with Newton and Leibnitz, has," he says, "been traced with fidelity and sustained interest by the hand of a master, whose calm judgment weighed impartially the different claims of discovery, whose powers of illustration could expand the fine results, and whose luminous eloquence was commensurate with the dignity of the subject." Nor is his observation on his own task less just; namely, that the more crowded field of discovery which it presents rendered it one of increased difficulty;—"its multifarious materials often lying scattered among the countless volumes of the Transactions of learned Societies." His arrangement of these materials, and his view of the whole subject, is comprehensive, vigorous, and spirited; and the greater ease and perspicuity of its style makes this the most agreeable perhaps of all his writings.

The volumes of the *Edinburgh Philosophical Journal*, published between 1824 and 1829, contain some small contributions, which may be here mentioned as also belonging to his latter years. The first consists of *Remarks on the Light of the Moon and of the Planets*; the second, of an *Enumeration of the Instruments requisite for Meteorological Observations*; the third, of a *Letter* *on the Goniometer;* and the last, of *Observations on the Theory of Compression, applied to discover the internal constitution of our Earth.* The characteristic boldness and the poetical dress of his speculations are abundantly displayed even in these small productions. In the first of them, he endeavours to shew that the moon is a *phosphorescent* substance, like the Bolognian stone; and he anticipates a period when "she will no longer cheer our nights by her soft and silvery beams; when she will become dim and wane, and seem almost blotted from the blue vault of heaven. To our most distant posterity," he adds, "this prospect is indeed gloomy; but other changes will rise to renovate and embellish the spectacle of the universe." In the last, he carries his reasonings to the startling conclusion, that the crust of the planet on which we tread includes "an immense concavity, not dark and dreary, as poets have fabled, but containing *light* in its most concentrated state, shining with intense refulgence and overpowering splendour!"

Early in the year 1832, which unhappily proved to be that of his death, he was, on the recommendation, we believe, of Lord Brougham, then Lord High Chancellor, created a Knight of the Guelphic Order. This honour was also conferred on several other distinguished men of science, about the same time. In a letter mentioning the occurrence, he says, "my holiday title is now of course Sir John; but I shall always retain an affection for my old distinction of PROFESSOR." He had but few other titular distinctions of any sort; for he was far from setting any value on those arising from fellowship with Scientific Bodies. He was a member of the Royal Society of Edinburgh, but not of that of London. The only distinction of this kind that he in the least degree prized was his being elected a Corresponding Member of the Royal Institute of France. This took place, with honour, in 1820; the choice, if we recollect rightly, having fallen upon him, in preference to others then proposed, by a majority of thirty-three out of thirty-seven votes.

For a few years before the fatal one above mentioned, his occupations had been agreeably diversified, by his attention to the improvement of a small estate, called Coates, situate near his native place, of which he made a purchase. Here, the house and garden being every way commodious and suitable, and surrounded with scenes endeared by his earliest recollections, he loved to reside; and even those of his friends who most regretted that his precious time should be wasted on rural occupations, could not but sympathize with his feelings, and rejoice that the honourable labours of the Philosopher had enabled him to secure such a retreat. No one could enjoy more vigorous or constant health; and, though of a corpulent habit of body, he was exceedingly active, and fond of exercise. His strength, and the longevity of his family—a circumstance on which he himself founded flattering hopes—alike gave promise of longer life; and it is melancholy to think that its close was but too probably hastened by one of his foibles—a contempt for medicine, and an unwillingness to think that he could be seriously ill. In the last days of October, whilst engaged in superintending some improvements on his grounds, he exposed himself to wet, and caught a severe cold. This was followed by erysipelas in one of his legs, which he neglected, and again imprudently exposed himself in the fields. He soon afterwards became dangerously ill, and expired at Coates, on the evening of Saturday the 3d of November 1832, in the sixty-seventh year of his age.

It has been well observed by Dr Johnson, that "of every great and eminent character, part breaks forth into public view, and part lies hid in domestic privacy. Those qualities which have been exerted in any known and lasting performances, may, at any distance of time, be traced and estimated; but those peculiarities which discriminate every man from all others, if they are not recorded by those whom personal knowledge enabled to observe them, are irrecoverably lost."* To prevent "this mutilation of character," as the same writer calls it, we shall close our narrative with a few details more particularly illustrative of the mind, opinions, and dispositions of this remarkable person. His discoveries, and the facts and controversies connected with them, may be discussed by many; and the full detail of them would require more space than can here be afforded; but, having enjoyed all the advantages for observation which long and intimate "personal knowledge" alone can supply, we think it right to endeavour, though briefly, to prevent some characteristic features from being "irrecoverably lost." The portrait may be imperfect; we cannot, indeed, complete it to our own satisfaction on our narrow canvass; but, in as far as the sketch extends, we can say that it is faithfully copied from nature.*

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* Johnson's *Life of Sir Thomas Browne.*

It would be impossible, we think, for any intelligent and well-constituted mind, thoroughly acquainted with the powers and attainments of Sir John Leslie, not to entertain a strong feeling of admiration for his vigorous and inventive genius, and of respect for that extensive and varied knowledge, which his active curiosity, his excursive reading, and his happy memory, had enabled him to amass and digest. Some few of his contemporaries in the same walks of science may have excelled him in profundity of understanding, in philosophical caution, and in logical accuracy; but we doubt if any surpassed him, whilst he must be allowed to have surpassed most, in that creative faculty—one of the highest and rarest of nature's gifts—which leads to, and is necessary for, discovery, though not all-sufficient of itself for the formation of safe conclusions; or in that subtilty and reach of discernment which seizes the finest and least obvious qualities and relations of things—which elicits the hidden secrets of nature, and ministers to new and unexpected combinations of her powers. "Discoveries in science," to use his own words, "are sometimes invidiously referred to mere fortuitous incidents. But the mixture of chance in this pursuit should not detract from the real merit of the invention. Such occurrences would pass unheeded by the bulk of men; and it is the eye of genius alone that can seize every casual glimpse, and discern the chain of consequences." With genius of this sort he was richly gifted. Results overlooked by others were by him perceived with a quickness approaching to intuition. To use a poetical expression of his own, they seemed "to blaze on his fancy." He possessed the inventive in a far higher degree of perfection than the judging and reasoning powers; and it thus sometimes happened, that his views and opinions were not only at variance with those of the majority of the learned, but inconsistent with one another.

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* The substance of some of the following observations appeared in the Newspapers immediately after the death of Sir John Leslie. They may, without impropriety, be used here, by the pen from which they originally proceeded.

Notwithstanding the contrary testimony, explicitly recorded, by the founders of the English Experimental School, he denied all merit and influence to the labours of the immortal delineator of the Inductive Logic. He freely derided the supposed utility of Metaphysical Science, without perceiving that his own observations on *Causation* virtually contained the important admission, that Physical is indebted to Mental Philosophy for the correct indication of its legitimate ends and boundaries. His writings are replete with bold imaginative suppositions; yet he laments the "ascendency which the passion for hypotheses has obtained in the world." * His credulity in matters of ordinary life was, to say the least of it, as conspicuous as his tendency to scepticism in science. It has been profoundly remarked by Mr Dugald Stewart, that "though the mathematician may be prevented, in his own pursuits, from going far astray, by the absurdities to which his errors lead him, he is seldom apt to be revolted by absurd conclusions in other matters." "Thus, even in physics," he adds, "mathematicians have been led to acquiesce in conclusions which appear ludicrous to men of different habits." Something of this sort was observable in the mind of this distinguished mathematician. He was apt, too, to indulge in unwarrantable applications of mathematical reasoning to subjects altogether foreign to the science; as when he finds an analogy between circulating decimals and the lengthened cycles of the seasons! But when the worst has been said, it must be allowed that genius has struck its captivating impress over all his works. Whether his bold speculations lead him to figure the earth as enclosing a stupendous concavity filled with light of overpowering splendour; or to predict the moon's arrival at an age when her "silvery beams" will become extinct; or to ascribe the phenomena of radiated heat to aerial pulsations; we at least perceive the workings and aspirations of a decidedly original and lofty mind. This, however, is not all. His theoretical notions may be thrown aside or condemned; but his exquisite instruments, and his experimental devices, will ever attest the eminent utility no less than the originality of his labours, and continue to act as helps to farther discovery. We have already alluded to the extent and excursiveness of his reading.

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* See Introduction *to Elements of Natural Philosophy.*

It is rare, indeed, to find a man of so much invention, and who himself valued the inventive far above all the other powers, possessing so vast a store of information. Nor was it in the wide field of science alone that its amplitude was conspicuous. It was so in regard to every subject that books have touched upon. In Scottish history, in particular, his knowledge was alike extensive and minute; and he had, in acquiring it, gone deep into sources of information—such as parish records, family papers, and criminal trials—which ordinary scholars never think of exploring. The ingenious mathematician, the original thinker and discoverer, the rich depositary of every known fact in the progress of science, would have appeared to any one ignorant of his name and character, and who happened to hear him talk on this subject, as a mere antiquary; or, at best, as a curious and indefatigable reader of history, whom nature had blessed with one strong faculty, that of memory. His conversation shewed none of that straining after "thoughts that breathe, and words that burn," so conspicuous in his writings. In point of expression, it was simple, unaffected, and remarkably correct. Though he did not shine in mixed society, and was latterly unfitted, by a considerable degree of deafness, for enjoying it, his conversation, when seated with one or two, was highly entertaining. It had no wit, little repartee, and no fine turns of any kind; but it had a strongly original and racy cast, and was replete with striking remarks and varied information.

Viewing his mind with reference to its moral attributes and habitudes, we must allow that it was not free from imperfections. He had prejudices, of which it would have been better to be rid; he was not over charitable in his views of human virtue; he was not so ready, on all occasions, to do justice to kindred merit as was to be expected in so ardent a worshipper of genius; and his care of his fortune went beyond what is seemly in a philosopher. But his infirmities were far more than compensated by his many good qualities;—by his equanimity, his cheerfulness, his simplicity of character, his straightforwardness, his perfect freedom from affectation, and his unconquerable good nature. He was, indeed, one of the most placable of human beings; and, notwithstanding his attention to his own interest, it is yet undeniable, that he was a warm and good friend, and a relation on whose affectionate assistance a firm reliance ever could be placed. He was fond of society, and greatly preferred and prized that of the intelligent and refined; but no man ever was more easily pleased; no fastidiousness ever interfered with his enjoyment of the passing hour; he could be happy, and never failed to converse in his usual way, though in the humblest company; and we have often known him pass an afternoon with mere boys, discoursing to them pleasantly upon all topics that presented themselves, just as if they had been his equals in age and attainments. He was thus greatly liked by many who knew nothing of his learning or science, except that he was famous for both.

He was never married. As to his person,—he was rather below the middle size, and corpulent, but well limbed; and though his face was large and florid, there was that about his eyes and forehead which seemed to shew that he was no ordinary man. There is a Bust of him by Joseph; a Portrait of the common size, taken a few years before his death, by Wilkie; and a Head, drawn at an earlier period, by Henning, which presents a very striking likeness.*

On Rhodizite, a New Mineral Species, from Russia.

In a recent number of Poggendorff's Journal, Gustav Rose has communicated some particulars regarding this new substance. Rhodizite has hitherto been found only in very small dodecahedral crystals, whose alternate three-sided solid angles are slightly truncated by the planes of a tetrahedron; and these planes are peculiarly smooth and shining, while the planes of the dodecahedron have somewhat less lustre, and are frequently uneven. The crystals are greyish or yellowish white; have a glassy, passing into an adamantine lustre; and are more or less translucent. Their hardness is considerable, for they scratch topaz, and therefore also boracite. The specific gravity of several separate crystals, which together weighed 0.386 grains, was found to be 3.415. By an alteration of temperature, the crystals exhibit strong polar electricity. The electrical axes, as in the boracite, unite the two opposite three-sided solid angles of the dodecahedron, and are therefore four in number; those angles on which there are tetrahedral faces exhibit positive electricity, and the others negative, during a diminution of temperature; but during an increase of temperature the case is reversed, for the first exhibit negative, and the last positive electricity. When exposed to the blowpipe, the rhodizite displays a bright green flame; but there is also a tinge of red. Professor Rose has not analysed the substance, as he expects to receive larger specimens; but a qualitative examination afforded no trace of lithion; and it is therefore to be supposed, either that the quantity in the very small portion examined was too minute for detection, or that, as is sometimes the case, the red tinge of the flame was derived from the lime.

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* In the last number of the Edinburgh Review, in an article on Dr Buckland's Bridgewater Treatise, a most erroneous account is given of the geological discussions that took place, many years ago, in the Royal Society of Edinburgh, and Professor Leslie figures there in the list of Huttonians, although it is well known that he opposed all the leading doctrines of the theory *of the earth* of Hutton.—Edit.

The electrical phenomena of the rhodizite give additional probability to the opinion, that not only in its crystalline form, but also in its atomic constitution, this mineral is identical with boracite, and hence that it is isomorphous with it. It is possible that the rhodizite is nothing else than a lime-boracite, just as the common boracite is a magnesia-boracite. It has hitherto been found in two localities, viz. at Sarapulsk, a village which, according to Georgi (*Physico-geographical and Natural-historical Description of the Russian Empire*, vol. iii. p. 189) is about 12 wersts from Mursinsk, a town about 100 wersts north from Catharinenburg; and at Schaitansk, situated about 72 wersts north from Catharinenburg. At the latter locality the crystals are larger, some having been found about two lines in diameter; at Berlin there is a specimen of the granite of this locality, on which the crystals of Rhodizite are partly superimposed on crystallized quartz, partly imbedded in red tourmaline, and partly contained in a clay occurring in the small cavities between the constituent parts of the matrix.

Comparative Remarks on the Distribution of Vegetation on the greatest Heights of the Himalayah and of Upper Peru. By J. MEYEN.* (Read before the Geographical Society of Berlin.)

I HAVE already, on one occasion, had the honour of directing the attention of the Society to that plateau on which there exist ruins of magnificent buildings, indicating the high cultivation of the inhabitants at a period very remote, and now almost beyond the reach of history. It was the fanatical Incas, erroneously represented in our histories and poetry, as mild and wise sovereigns, who with conquering hands destroyed the high degree of civilization of the people of the plateaus of Chuquito and Tiahuanaca. The present memoir does not refer to those works of art, which, after an existence of a few centuries, are now nearly totally destroyed; but to that eternal delightful covering of our planet, whose character is with difficulty altered even by the most abundant population.

A perpetual spring prevails on the plateau of Chuquito; snow is a rare phenomenon, and yet this fruitful region lies beyond the natural boundaries of trees. The great lake of the country, situated at a height of 12,700 English feet, is traversed by canoes made of rushes at all seasons of the year, and, nevertheless, wheat and rye do not ripen. Maize, which in Europe does not extend to the sub-arctic zone, is not cultivated on the banks of the lake; but, under particular precautions, ripens on the island of Titicaca, in the middle of the lake. This maize, was consecrated in former times, and was carried to all parts of the Peruvian empire by virgins dedicated to the worship of the sun. These few facts are sufficient to authorize the observation, that there must prevail on the plateau of Chuquito a climate very peculiar, and widely different from that belonging to the corresponding zones of northern Europe.

If we compare the vegetation of the district now before us, in regard to its physiognomy, with that of European districts, we shall find that it corresponds to the region of the alpine roses, or to that of shrubs in our mountain vegetation; and that it resembles the vegetation of the most southern portion of the arctic zone in the vegetation of the plains. But the strong contrasts of the seasons with their great differences of temperature, which occur in the northern zone, and the very equable temperature of the lake of Titicaca, must naturally give rise to very great distinctions in regard to the vegetation of the corresponding regions. The most important circumstance, however, in forming our estimate of the climate of the plateau of Upper Peru, is the remarkable phenomenon, that there, the winter, that is, the period from May to November, is not only the dry season, but also the warm one; while, in the real summer, *according* to the position of the sun, that is, from November to April, the wet, and, at the same time, the colder weather prevail. During the latter hardly a day passes without rain, and falls of hail and snow, which are so very rarely observed, take place in November and December, the very months which ought to be the hottest of the whole year.

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* From Wiegmann's *Archiv für Naturgeschichte,* 1836.

This extraordinary inversion of the usual meteorological relations produced by the position of the sun, is certainly a phenomenon well worthy of attention, not only for meteorology, but also for the physiognomy of vegetation, and the social condition of human society. It is evident that since the winter, according to the position of the sun, is the dry season, the temperature at a particular place, owing to the constant clearness of the heavens, and the consequent opportunity afforded for the increase of heat by means of the sun's rays, must be much higher than could belong to it according to general laws. On the contrary, and this is of the greatest consequence for the vegetation, the summer is so much the colder, because, owing to the constant rains and the evaporation of the fallen water, the refrigeration of the air ensues; and this chiefly because the very foggy atmosphere prevents the passage of the sun's rays. It is also sufficiently well known, that at such great heights on the mountains, whenever the influence of the sun is wanting, a very low temperature is immediately the consequence.

Thus, then, on the extensive plateau of Upper Peru, the climate is in this remarkable manner altered to the prejudice of the vegetation, and all its dependent relations; and we need not therefore wonder that the height of the snow-line, and that of the highest vegetation in the country, should not rise 2000 feet higher than on the Himalayah range; the difference of the latitude of the two mountain masses indicating exactly this difference in height of 2000 feet.

It is generally regarded as an ascertained fact, that the vegetation reaches a higher level on the Himalayah than on any other mountains of the earth; but, as I shall immediately show, this conclusion ought to be very much limited. The boundary of eternal snow is certainly very high in some portions of the Himalayah, especially in north-eastern Kunawar; no less a height can be assigned to it than 17,000 feet, although at some points snow is found at a somewhat lower level. At the pass of Keubrung, at a height of 18,300 feet, only a little snow was met with, and the heat of the sun during the summer was extremely oppressive; Gerard found no snow at one place in north-eastern Kunawar at a height of 20,000 feet; and on the mountains of the Plateau which proceeds towards Tartary, and which itself has a height of 16,000 feet, no snow was met with at a height of 19,000 feet; indeed the snow-line is so elevated at these passes, that travellers cross them during summer and winter. It is much to be regretted, that no hygrometrical observations have been made at these heights, and that we possess no information as to the winds; an extraordinary dryness of the air must evidently be the cause of the want of the snow, and these very great heights of the snow-line are by no means to be regarded as normal. This great elevation of the snow-line is so much the more striking, because the mountain masses lie at the most northern limit of the sub-tropical zone, where, therefore, according to the relative position of the sun to the earth, the snow-line ought to be lower than within the tropics. If we compare with these results the heights of the snow-line in the Peruvian Cordillera, we find, according to the already existing numerous observations, that, in general, for the summits of the simple chain, the height of the snow-line is to be estimated at from 15,700 to 16,000 feet, according to Humboldt and Basil Hall; but that for the summits and the plateau in southern Peru, the height amounts to 16,500 and even 17,351 feet. The volcano of Arequipa has a height of more than 18,000 feet, and yet it is only on the southern summit that there is a trace of snow.

We see therefore, that also in the Peruvian Cordillera, at those points where great mountain masses include extensive flat tracts, the limit of eternal snow ascends much beyond the usual heights, and corresponds with that in the northern Himalayah, where still larger mountain masses are grouped together; and that there also, that is in the Cordillera, isolated and extremely remarkable exceptions occur, although a very large portion of the range of the Cordillera, especially the part in southern Bolivia, where the great volcano of Gualatieri is situated, is still quite unknown.

But in the Himalayah, there are also a great many places where the snow limit ascends far above 17,000 feet! All this taken together, leads to the conclusion, that the dome-like curve of the line of eternal snow, when drawn so as to include the whole earth, must, according to the observations we possess, be in the highest degree uneven. We could, it is true, calculate a curve from the relative position of the sun and the earth, where the temperature would be that of the freezing point; but this does not coincide with the snow line, and besides would not coincide with the points of the snow line as actually observed on the earth.

Having thus shewn a great agreement in the height of the snow line of the two highest and most extensive masses of mountains, viz. of the Peruvian Cordillera and the Himalayah, I shall now compare the heights to which vegetation reaches on these mountains.

In the year 1831, Captain Hall observed on Chimborazo, at a height of 17,000 feet, several beautiful alpine plants in flower; and I have shewn that even the shrubby vegetation, therefore the region under that of alpine plants, occurs in the high mountains of southern Peru at a height of 15,500 to 16,000 feet. In the Himalayah, on the contrary, and that only in north-eastern Kunawar, the vegetation extends to 16,000 feet; but I know no example there where the alpine plants pass that elevation, as, for example, they do on Chimborazo, on the volcano of Arequipa, and several other places in the Peruvian Cordillera. At a height of 15,225 feet, there have been found on the Himalayah, species of *genista* and *astragalus*, together with the *rheum emodi*, one of the true rhubarbs, and species of *pedicularis* and *primula*; but luxurious compared with this, is the vegetation on the pass *Los Altos de Toledo* in the Peruvian chain, where *syngenesian* resinous shrubs vegetate at an elevation of 15,500 feet. At such heights, and beyond them, there occur on the Himalayah, only mosses and grasses, and no trace of shrubs. At a height of 15,000 feet, there are marshes with bushes; the *Juniperus excelsa* and *J. recurva* are found only as high as 14,500 feet, while barley is reaped at a height of 14,900 feet. At 14,700 feet, Gerard found a village in north-eastern Kunawar, where the temperature in the middle of October was 17° F. in the morning, and the stream was not free of the ice that had been formed during the night till 2 o'clock P.M. The birch and *rhododendron lepidotum* extend beyond 14,000 feet; and to the east of Dabling, in north-eastern Kunawar, there is cultivation at 13,600 feet. At that spot there are fields of barley, buckwheat, and turnips. In most other places, the cultivation does not ascend beyond a height of 11,500 or 12,000 feet. The highest limit of the pine is 12,300 feet: the pine woods do not extend beyond 11,000 to 11,800 feet; but at a much higher elevation poplars of 12 feet in circumference have been observed.*

We have already seen, that on the plateau of Chuquito, only the barley and oats ripened at a height of 12,700 to 12,800 feet, and this may be easily explained. On the banks of the lake of Titicaca there prevails constant spring weather, that is, a temperature which, during the whole year, deviates but little from that which belongs to our spring; and while the average winter temperature is higher, the average summer temperature is lower. And it is this summer temperature, when it continues to the time of ripening, which alone favours the cultivation of the *cerealia*. But, as the summer heat is so very low in these districts, rye and wheat do not ripen. It at first appears remarkable that excellent potatoes are cultivated close to the fields of rye and barley, while with us it is quite common to find the young potato plants destroyed by a degree of cold which would not at all injure fields of grain. This, however, just affords additional proof of my original proposition; for it thus appears that the temperature in the warm season is never so low as to destroy potatoes, but is unfortunately never so high as to admit of the ripening of wheat. The potato does not require so high a temperature as the finer kinds of grain, but it requires a longer period of a corresponding lower temperature than those annual grasses, some of which, in a very elevated summer temperature, ripen in a space of three weeks.

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* Asiatic Journal, May, 1825. P. 629.

The plateau of Chuquito, round the great lake, is naturally destitute of trees, although it has only a height of 12,700 feet; hence it might be believed that the climate is severe, and it might be concluded, that, on this account, the finer grains would not ripen; but the whole phenomenon is local, and the absence of trees is, as we may say, an accident produced by peculiar causes, just as we find such an occurrence in the vegetation of plains. Thus, the Falkland Islands lie in a zone which corresponds perfectly to the subarctic zone of northern Europe, and the physiognomy of the vegetation of these islands bears the most striking resemblance to that of northern Denmark and southern Sweden and Norway, but trees are awanting. That the want of trees is there a local phenomenon, is proved by their abundant occurrence at no great distance, and in a more southern latitude, viz., on both sides of the Straits of Magellan, where evergreen box-trees, with trunks having a circumference of 12 to 17 feet, form most luxuriant forests.

In the Northern Hemisphere, especially in northern Europe, evergreen-trees as well as shrubs, follow more or less, the sea coast; and in southern Europe, where all the countries, by their peculiar configuration, are surrounded by the sea, we have a great predominance of these vegetable forms. In the Southern Hemisphere, on the contrary, the appearance of evergreen-trees and shrubs is quite an unusual phenomenon; perhaps such vegetable productions stand in more intimate connexion than we suppose, with the predominating influence of the sea. Here it is not only the subtropical zone, not only the warmer portion of the temperate zone, which corresponds to our southern Europe, but we find this form of trees with evergreen-leaves extending even to Magellan's Straits and beyond them; and our tender-leaved box of northern Europe corresponds to the evergreen box-trees o

South America, the southern part of New Holland, Van Diemen's Land, New Zealand, &c.

Many splendid cities existed on this plateau of Chuquito; and their temples are among the most magnificent I have seen in South America. Large villages, with the remains of vast monasteries, are situated in this district, which was once the seat of agriculture and of the fine arts; but its inhabitants never experience the shade of trees, and hence an everlasting spring-climate, although so exquisite to the imagination of the poet, and although in our cold climate it delights us so much after the long winter, is yet united with many and great deprivations. It is to be hoped, that the great work now being published by D'Orbigny, who has enjoyed the rare good fortune of spending a considerable time on the eastern bank of the lake of Titicaca, will throw great light on this hitherto very imperfectly known region. The beautiful map of the district, which this indefatigable traveller has published, is one of the most important modern additions to our geographical knowledge; it proves that the eastern side of the lake is as abundantly peopled as the western, which latter I was able to visit but for a few hours. Relations of this kind, and at such a height, are entirely unknown in the Himalayah; they might be expected at a height of 10,000 feet, owing to the higher latitude of the mountains compared with the plateau of upper Peru; but this is, in fact, not the case.

It is not merely a supposition, that the want of trees on the plateau of Chuquito is to be regarded as a local phenomenon; for that such is the case, is fully proved by the following observations. It is a well known fact that our fruit trees, such as apples, pears, and quinces, are much less calculated to resist a rough climate than fir-trees, beeches, and other forest trees of a similar description; and yet we actually find these fruit trees in the gardens of the towns of Puno, Chuquito, Acona, &c., where they have reached a height not inferior to that of those in our gardens. In the secluded ravines which are not reached by the sun, apples and quinces are ripened; but they taste, as might be expected, not better than if they were ripened at Christiania. A beautiful little tree,—a buddlea, quite covered with golden-red bunches of blossom, is sometimes the ornament of the gardens; it has been brought from Bolivia, and would form a great addition to the summer flora of our gardens, but would require protection in winter.

It is difficult, indeed impossible, to determine for the western side the height to which the tree vegetation reaches in southern Peru, because the acclivity of the Cordillera is too steep, and at the same time devoid of soil. In northern Peru and in Quito, tree-shrubs extend to a height of 13,000 feet. Tunguragua is covered with shrubs at a height of 13,317 feet; but at another place, on the eastern side, towards Maranon, woody plants occur nearly at a height of 14,000 feet; these plants, however, belong to the region of shrubs which again passes into the region of alpine plants. The same remark holds good regarding an observation occurring in Pœppig's account of his tour.* It is there stated, on the authority of a report on the Canal of Tacna, by B. Scott, an engineer, that on the plateau which may be termed the Plateau of Tacora, very considerable woods occur at a height of 14,899 feet; that the *Cienega de Nohusuma* is partly surrounded by trees belonging to these woods, although, according to Mr Scott's own observations, it lies at a height of 14,930 English feet. It is there said, that even the northern acclivity of the snowy mountain of Tacora is covered to the same height with similar trees. As I have myself seen all these places, and have formed an entirely different idea of their vegetation, it is necessary that I should offer some remarks on the subject. The account by Mr Scott was published by Pœppig about a year and a half after the appearance of the report of my journey; it may hence be regarded as a newer and more correct description; but I must repeat, that on the whole plateau of Tacra, although I always travelled during the day, and had very clear weather, I did not see a single tree, not even a tree-like shrub. It is only low bushes, mostly syngenesian, of remarkable forms and abundantly covered with resin, that constitute the woody vegetation of this plateau, which reaches a height of 14,800 to 16,000 feet (as at the water-shed between Rio Uchusuma and the Rio Moure). Small thorny shrubs of the Solanaceæ belonging to the rarest species of this family, Leguminosæ with juniper-like leaves, Wilsoniæ, the knotty Magericarpus, &c., are next to the Syngenesian plants in number. It is, therefore, not surprising, that Mr Scott, who has lived so many years in that treeless region, should give the name of trees to such small shrubs, since the value of wood in such a country is extremely great.

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* Vol. ii. p. 80.

It appears to me, that the planting woods on the Plateau of Chuquito would be quite possible, and that for this purpose it would be advisable to select such trees as flourish in the vicinity of the polar circle. This plan was proposed by Scholtz of Breslau, who resided many years at Lima, and has distinguished himself by many useful memoirs. Large quantities of seeds have been sent there; they were sown, but unfortunately none have succeeded. The climate itself, as we would willingly suppose, is certainly not to be blamed, but only the little care which has been bestowed on the planting; and it is much to be wished for the benefit of the inhabitants, that other friends of humanity would interest themselves in this matter. On the other hand, the planting operations carried on by Scholtz in several other places of the higher regions of Peru, as, for example, on the Plateau of Pasco, have had the most favourable result.

It would then seem probable, not only that the vegetation, especially of the larger description, occurs at a lower level on the Himalayah than on the Cordillera of South Peru, but also that in the latter there are some localities, which surpass, in a most remarkable manner, all similar phenomena in the Himalayah. In conclusion, let us cast a glance at the vegetation in general, which covers the greatest heights on those so widely separated points on the earth's surface, in order to indicate their analogies as well as their differences.

When we consider, in a general point of view, the physiognomy of the vegetation of the northern half of the globe, we arrive at the conclusion, that it varies considerably according to latitude, but that the variation is, in fact, very slight according to a change of meridian. It is the trees and shrubs which, by their physiognomy, chiefly communicate character to the vegetation of a country; and it is on the mode of their reciprocal distribution; on the arrangement in series, one with another, of the different forms of these plants; and on their alternation with meadows and smaller vegetable forms, that there depend the most characteristic features of the vegetation of different regions. There is no doubt that vegetation, not only in a physiognomical, but also in a statistical point of view, is uniform in the whole northern portion of the northern hemisphere; and this similarity, apart, of course, from local phenomena, extends nearly to the middle of the temperate zone. The same vegetable forms,—the same families,—nay even nearly the identical genera and species, occur there, as well in America as in Asia and in Europe; and also the mode of their combination is almost every where the same. Proceeding southwards, the vegetable forms accumulate; and the number of those that are peculiar to this or that meridian becomes greater; but the physiognomy, nevertheless, remains wonderfully similar. In the colder portion of the temperate zone, our beautiful forest trees (*Laubhölzer*), predominate both in Europe and in North America. In the warmer portions of the temperate zone, and also in the subtropical zone, there prevail the evergreens (*Laubhölzer mit immergrünenden Blättern*); and these present themselves in America as well as in Europe and Asia, although, in reference to the genera, important differences occur in different meridians. It has been attempted to distinguish the territory of the Magnolias, that of the Camellias, &c., but such territories have not been well established; there are only particular genera belonging to these vegetable forms, which, in certain longitudes of the corresponding zone, are represented by other genera. The magnolias include only a few species, and do not occur in large masses, like our forest trees; they appear on the east side of Asia, viz. in Japan, where they are associated with camellias. In the same latitude in Europe and the neighbouring portions of Africa, we have laurels, myrtles, the quercus ilex, the pistachio, and the arbutus unedo; and we want only the large flowers and large leaves of the magnolias; the cypress form, on the contrary, extends from America to the east coast of Asia, and with it are associated heaths and tamarisks. The knotty and thorny shrubs, and the tree-like grasses, which make their appearance in Southern Europe, have corresponding representatives in the same zone in North America and in Asia; in short, I could follow out this similarity in the physiognomy of the vegetations much farther, even to the most special cases. Even the palms which reach this zone have the same forms.

But we find also the same features in the northern hemisphere, when we compare the vegetation of the different regions of the mountain flora with that of the corresponding zones. The physiognomy of the vegetation remains the same, and only particular genera and families exhibit the peculiarity of ascending to the highest regions of the mountains in the south, and yet being altogether awanting in the corresponding zones towards the north. A particular comparison of the alpine flora of the Himalayah, (as we find it given in the excellent works we now possess on the subject,) with the vegetation of the corresponding northern zones, has convinced me that there is almost no difference either in the physiognomy of the vegetation, or in a statistical point of view. There are, it is true, many species peculiar to these mountains; but the genera are nearly all the same, and the species have extremely similar and completely corresponding forms in northern Europe. This similarity, in fact, goes very far; at a height of from 11,000 to 12,600 feet, there prevails on the Himalayah a flora which is entirely similar to that of the Scandinavian peninsula; and at a height of from 7000 to 8000 feet our forest trees predominate, which, though differing in species, have nevertheless the same physiognomy as in Germany. Nay, this similarity to the European flora proceeds still further. The valley of Cashmere, placed at the boundary of the subtropical zone, presents an oval plain, which is situated at a height of from 5200 to 5500 feet above the level of the sea, and is surrounded by lofty mountains. It is abundantly watered, being traversed by lakes, streams, and canals; and presents a luxuriant vegetation, which is rich in evergreens. Rice and melons, it is true, are cultivated during summer in Cashmere, but the flora exhibits quite the same species that occur in Germany. The cultivation of walnut-trees is carried on to a great extent, and poplars and fruit-trees flourish extremely well. The vine there twines itself round the poplars, and the grape is employed for making wine and raisins; and our water-nut (*Tribulus*) corresponds to another similar species in the lakes of Cashmere, whose fruit is well known as a common article of food of the inhabitants of the plateau.

But it is entirely different with the vegetation of the southern hemisphere of our planet. It is a striking and inexplicable phenomenon, that there the vegetation exhibits remarkable differences, not only according to a change of latitude, but also according to a change of longitude, without at all taking into consideration the great difference of character in the vegetation of the southern hemisphere from that of the north; and that, in so far at least as the higher latitudes are concerned, there are in reality in the northern hemisphere only *representatives* of the vegetable forms of the corresponding zones of the southern hemisphere. And the same is the case *vice versa;* for there occur also in the higher latitudes of the southern hemisphere representatives of the corresponding latitudes of the northern hemisphere. And, in the same manner as we find the vegetation of the plains of the southern hemisphere, so is it also on the highest mountains; and also in those districts of Upper Peru from which we started. The vegetation of the heights of Upper Peru has hardly any resemblance to that of the Himalayah: there we hardly find representatives of those genera which in the Himalayah, and generally in the mountains of the northern hemisphere, form the alpine vegetation. On the contrary, there occur distinct forms of genera and families, which are partly quite foreign to our northern hemisphere, and partly belong only to its more southern portions, and never present themselves in the highest latitudes, or ascend to the highest regions of our mountains. To the exquisite primroses of the alpine flora of our northern hemisphere there correspond the singularly modelled form of the Merlineæ and that of the Verbenaceæ. The genera *Mimulus*, *Alstræmeria*, *Calceolaria*, *Tropæolum*, *Calandrinia*, and *Adesmia*, which now form the greatest ornaments of our gardens, are sometimes collected into the most enlivening patches close to the limits of perpetual snow; and the genera *Espeletia*, *Oxalis*, *Acæna*, *Nierembergia*, *Atropa*, *Lycium*, *Culcitium*, *Chuquiraga*, *Sida*, and many others, contribute to clothe the region of alpine plants; while of all these genera not a trace is found in the region of alpine plants in the northern hemisphere. The genus *Sida*, and the Malvaceæ generally, remain at a distance from the Arctic zone in our hemisphere, and do not ascend to the region of the alpine plants; while on the Peruvian Cordillera they extend to the limits of perpetual snow, and there actually constitute the most remarkable forms.

Some Remarks on an article of Mr John Davy, M. D., F.R.S. inserted in the Edinburgh New Philosophical Journal, April –July 1834, *p.* 42, *&c., on the Supposed Property of Tin of Preserving Iron from Corrosion in Sea-Water.* By A. VAN BEEK, Member of the Royal Institute of the Netherlands. Communicated by the Author.

SOME time ago, while perusing the Edinburgh New Philosophical Journal for 1834, I met with an article of Mr John Davy, M. D., F. R. S., from Malta, entitled " Some Observations on a note of Mr A. Van Beek, purporting to point out an error in the Bakerian Lecture of the late Sir Humphrey Davy, 'On the Relation of Electrical and Chemical Changes.' "

This note occurring in my memoir, " Sur un phénomène extraordinaire concernant l'influence continue qu'exerce le contact de métaux hétérogènes sur leurs propriétés chimiques, longtems après que ce contact a cessé," inserted in the 38th volume of the Annales de Chimie et de Physique, par M.M. Gay Lussac et Arago, is of the following tenor :—" Dans le cours de mes expériences sur la préservation des métaux, je me suis aperçu d'une erreur grave, que le célèbre chimiste Anglais Sir Humphrey Davy a commis, dans le Bakerian lecture du 8 Juin 1826," " On the Relation of Electrical and Chemical Changes," " publié dans les Transactions Philosophiques de 1826, où il recommande d'employer le zinc *ou l'étain* pour la préservation des chaudières à vapeur, surtout celles des bateaux à vapeur, où l'on fait souvent usage de l'eau de mer ; des expériences décisives m'ayant appris que l'étain, bien loin de preserver le fer, est au contraire préservé par ce dernier métal ; et qu'ainsi un morceau d'étain introduit dans la chaudière, au lieu de preserver le fer d'oxidation et de diminuer par là les dangers d'explosion, devrait puissamment contribuer à sa prompte corrosion. Si l'on peut faire usage de cette application utile du principe de la préservation réciproque des métaux, le zinc seul devra être employé."

In the above-mentioned article, Mr John Davy endeavours to vindicate the opinion of his late celebrated brother, Sir Humphrey Davy, recommending the use of tin for preserving the iron-boilers of steam-vessels from corrosion by sea-water; and to shew that I was in error in the remarks which I had made.

The article by Dr Davy is written with all the moderation and politeness which ought to characterize the refined cultivator of science; and such being the case, it becomes a real pleasure to discuss a disputed point with candour and impartiality.

Being myself a true admirer of the eminent merits of the late Sir Humphrey Davy, whose discoveries, with regard to their useful application in navigation, I have endeavoured myself strongly to promote among my countrymen; it was neither hyper-criticism nor unfounded suspicions, but only the love of truth, that directed my pen in writing the note in question. Moreover, being fully convinced, from accurate and decisive experiments, I thought it my duty to warn those who, on the authority of Sir Humphrey Davy, might employ tin instead of zinc to protect the iron-boilers of steam-vessels from corrosion by sea-water. I resolved, nevertheless, to re-examine, with the utmost care and impartiality, the whole question, and to publish whatever might be the result of my investigation.

The following experiments were made on this subject:—

1. A piece of iron, of 65^{mm} square, was placed in a cylindrical vessel containing about half a litre of sea-water, and the iron was quickly corroded. After forty-two days the whole plate was oxidated, and a strong precipitate of oxide of iron lay at the bottom of the glass-vessel.

2. A similar plate of iron, on the surface of which was attached a small piece of tin of 23^{mm} square, was at the same time exposed to a similar quantity of sea-water. In a few days the iron was strongly oxidated; and the sea-water turned to a red colour owing to oxide of iron, which increased every day, and formed, as in the first experiment, a thick sediment at the bottom of the glass; whilst the tin, as far as its surface was not covered by precipitated oxide of iron, remained bright, which was principally to be observed on the sides or edges of the small tin plate, on which the oxide of iron could not remain stationary. In forty-two days the quantity of oxide of iron in this vessel was by no means less than that of the preceding experiment.

3. A plate of tin, 65$^{m\,m}$ square, on which was fixed a small piece of iron plate of 23$^{m\,m}$ square, being in the same manner exposed to the action of sea-water, the iron was in a very short time oxidated, whilst the tin remained perfectly bright, without any trace of oxide of tin in the vessel. The quantity of oxide derived from this small piece of iron, was more than the double of that produced in the same time from the much larger iron plate of the first experiment.

4. By exposing a perfectly similar combination of tin and iron to the action of sea-water, the immediate contact of the two metals being only prevented by a thin plate of mica placed between both; in eight days the iron was but slightly oxidated, whilst the oxidation of the tin plate was evident.

5. A similar combination of tin, mica, and iron (but by which the two metals were brought in conjunction by a thin platina wire round the mica), being exposed in sea-water, the iron was again soon oxidated, though, as it seemed, in a smaller degree than that of the third experiment. In eight days, there was already much oxide of iron deposited on the sides and the bottom of the glass, whilst the tin did not shew any oxidation.

6. A plate of tin, in the same manner placed in sea-water, very soon produced traces of oxidation, principally on the upper side of the plate, which was placed obliquely against the sides of the glass-vessel; and the oxidation was chiefly perceived on the points where the surface of the tin-plate shewed some irregularities. The quantity of oxide obtained in this manner in forty-two days, was but small compared to the quantity of the oxide of iron of the preceding experiments.

7. A plate of iron, as in the first experiment, on whose surface was fixed a small piece of zinc-plate, on exposure to sea-water, remained constantly bright at the expense of the zinc, which was strongly oxidated.

The results of these experiments are in perfect accordance with those formerly obtained. The 2d experiment shewed that iron which, when exposed alone to sea-water, is speedily and strongly corroded, was by no means preserved from oxidation by tin; whilst by the 3d experiment it is proved that, on the contrary, tin which, when exposed alone to sea-water, as was shewn by the 6th experiment, is easily corroded, was preserved from it by iron. The 7th experiment finally, shewed evidently the eminent property of zinc to protect iron from corrosion in sea-water.

I can, therefore, consistently with truth, by no means retract any part of my memoir, being still strongly convinced, that tin cannot be made use of to preserve the iron-boilers of steam-vessels from corrosion by sea-water; whilst, on the contrary, zinc is perfectly adapted for that purpose.

But I proceeded further, in asserting that tin in the iron-boilers, instead of preserving the iron, would greatly increase the danger, by promoting the speedy oxidation of that metal. This fact is, as I suppose, entirely placed beyond doubt, by the results of my above-mentioned experiments; for, if the comparison of the 1st and 2d experiment should still have left any doubt on this head, the great quantity of oxide derived from the small piece of iron in the 3d experiment, compared with the far smaller quantity of the same matter, obtained at the same time from the much larger iron-plates in my 1st and 2d experiments, proves sufficiently the pernicious influence of tin in this respect.

The 4th experiment shews evidently the powerful influence of contact on mutual preservation of metals; but we learn at once that this influence only consists in the fact of an electrical conduction between the two metals, so that the immediate contact seems not to be absolutely necessary to preservation; for, according to the fifth experiment, the preservation of the tin by the iron, and consequently the oxidation of the iron, was in a great measure restored, by establishing, by means of a third metal, the electrical conduction between the two metals which had been separated by the plate of mica.

Mr John Davy, fully avowing the fitness of zinc to protect iron from corrosion in sea-water, has remarked, that, by using this mode of preservation for iron steam-boilers, it is to be questioned whether danger may not arise from the inflammable gas mixing with the steam. But besides the very small quantity of that gas, which can never increase indefinitely in the boiler, on account of its escaping from time to time together with the steam, I think that hydrogen gas, even in a great quantity, included in a steam-boiler, will not be more dangerous there than the same quantity in every closed gasometer, and can, when mixed with steam, by no means cause an explosion. When the bottom or the sides of a steam-boiler are incandescent for want of water, the boiler may undoubtedly burst, as dreadful experience has often taught; but in this case it is only the suddenly increased tension of the steam that makes the danger imminent, even without the presence of any inflammable or explosive gas.

On the contrary, I perfectly agree with Dr Davy in his statement, that the protection of iron-boilers of steam-vessels is not a matter of absolute necessity, since experience has taught us that the oxidation of the iron cannot take place after the sea-water boils, a circumstance which generally occurs. According to his experiments, which we found perfectly confirmed, the iron in sea-water is not oxidated by decomposition of sea-water, but chiefly by the air contained in it, and when this is expelled by means of an air-pump, or by boiling, the oxidation of the iron immediately ceases. I can, nevertheless, by no means conceive why Dr Davy finds in this circumstance, which he seems to consider as a peculiar case, the satisfactory explanation of the result of an experiment perfectly according with my experiments above described, but in apparent contradiction to his former investigations; in which experiment he observed, to his great surprise, that steel was not preserved by tin from corrosion in sea-water, whilst repeated galvanometrical experiments had taught him that iron in this fluid *must be* necessarily preserved by tin. I can, moreover, by no means agree with Dr Davy in his statement, that iron in sea-water would be in a similar case as when this metal is exposed to the action of humid air or acid vapours, since in this latter case, as he affirms, it cannot be perfectly preserved from oxidation by the contact of a more positive metal (zinc). I cannot participate in this opinion, *firstly*, because decisive experiments have taught me, that iron can certainly be completely preserved from oxidation in sea-water by a more positive metal (zinc); and, *secondly*, because copper, whose oxidation in sea-water proceeds exactly in the same manner, is also perfectly preserved from oxidation in this fluid by more positive metals (iron or zinc). The late Sir Humphrey Davy had already observed, that the copper sheeting of sea-vessels, to whose preservation he chiefly intended to apply his interesting discoveries, is not oxidated by decomposition of the sea-water, but only by the oxygen of the atmosphere always taken up by the water in a certain quantity, in combination with the carbonic acid gas of the air contained in it in the same manner. Copper, placed by this eminent philosopher in sea-water from which all the air was expelled by means of an air-pump, remained perfectly bright.

Dr John Davy principally endeavours to confirm his opinion against me by theoretical views, derived from the mutual electrical relation of metals; stating that his galvanometrical experiments always shewed him tin as positive in its electrical relation to iron, and that, therefore, iron must be defended by tin.

Owing to the want of a good galvanometer, I was not able, in the course of my former experiments, to make these inquiries, and I was now therefore anxious to determine the point to which I have just alluded.

I must confess that, at first, to my great surprise, I found the fact as Dr Davy states, observing the astatic needles of the galvanometer, by the immersion of iron and tin in sea-water, to shew immediately a deviation corresponding with a positive electrical relation of tin to iron.

It was principally the sagacity of my friend Mr G. T. Mulder of Rotterdam, which made me notice, that, by prolonging the experiment during a certain time, the needles always return to zero, and afterwards shew an opposite deviation, the tin acquiring a negative electrical relation to iron.

The galvanometrical researches have taught us, as a constant law, that where two metals are immersed together in a fluid, the metal that suffers the strongest oxidation is always positive in its electrical relation to the other less oxidated metal. Yet, where tin and iron or steel are both placed in atmospherical air, the tin is, without any doubt, positive in respect to the iron; for both metals being perfectly polished and bright, the tin is almost directly tarnished, and its surface becomes covered with a thin layer of the oxide of tin, whilst the steel remains still perfectly bright. Now, it is a curious circumstance, that this electrical relation of the two metals, acquired in the atmospherical air, subsists during a certain time after their immersion in sea-water, in consequence of a singular property; and it thus appears, that metals retain, for a longer or shorter time, the electrical state once acquired. I discovered this interesting fact, by making experiments on the preservation of copper by iron in sea-water, and it forms the contents of my memoir containing the note under discussion.*

Very soon, however, the needles of the galvanometer, after having returned to zero, shew an opposite deviation, and thus prove evidently, that the electrical relation of the two metals is wholly changed. The iron being more strongly oxidated than tin in sea-water, becomes positive in respect to this metal; and this positive electrical relation of the iron remains and increases by the increasing oxidation of this metal in sea-water.

The time, during which tin preserves its positive electrical relation to iron acquired in common air after the immersion in sea-water, depends on the quality of the iron, and, perhaps, still more on the condition of its surface, and the temperature of the sea-water. In general, the brightest surface of iron retains the longest its primitive electrical state, resisting powerfully the stronger oxidation which it must soon undergo in sea-water.

Generally, this phenomenon lasts only for a few minutes. I have seen it once continue during about half an hour, when making the experiment with bright polished steel, by a mean temperature.

The chemical purity of tin seems not to have any influence on these experiments.

It appears evident, that Dr John Davy, by his galvanometrical experiments, attended, as I did myself at first, only to the direction of the needles soon after the immersion of the metals in sea-water, neglecting to extend the experiment during a certain time, otherwise he would doubtless have remarked the opposite deviation of the needles.

As to me, I am now perfectly convinced of the fact, that tin cannot be made use of with success, to protect iron from corrosion in sea-water; tin, on the contrary, being in that case protected by iron; and I invite all philosophers to repeat my experiments, and so convince themselves of what I have advanced.

It was very agreeable to me that an eminent philosopher of the same name, Professor Edmund Davy of Dublin, was, on this subject, of the same opinion as myself. In an article "On some recent Experiments, made with a view to protect Tin-Plate or Tinned-Iron from corrosion in sea-water," inserted in the London and Edinburgh Philosophical Magazine, Nov. 1835, p. 391, we read :—" If a piece of tin-plate is exposed in sea-water for a few days, it will exhibit an incipient oxidation, which will gradually increase; the tin will be preserved at the expense of the iron, which will be corroded; but if a small surface of zinc is attached to a piece of tin-plate and immersed in sea-water, both the tin and iron will be preserved, whilst the zinc will be oxidated."

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* Professor A. de la Rive has discovered a similar property of the metals by a different process.

The following experiments, in which an accurate weighing of the metals had taken place, communicated to me, by my friend Mr G. T. Mulder of Rotterdam, are also in perfect accordance with my own experience.

1. A plate of iron, weighing 32.907 grs., was placed in a glass vessel containing one litre of sea-water, during twenty days, at the temperature of the month of November of last year.

After the experiment the weight of the iron was found to be, 32.726 grs.; loss by oxidation, 0.181 grs.

2. A similar plate of iron, exactly of the same weight of 32.907 grs., but on whose surface was fixed a small piece of tin weighing 8.140 grs., was in the same manner exposed during twenty days, in one litre of sea-water.

The weight of the iron after the experiment was found to be, 32.674 grs.; that of the tin, 8.139 grs.; loss by oxidation of the iron, 0.233 grs.; loss by oxidation of the tin, 0.001 grs.

The results of these experiments shew, that the iron, when exposed alone to sea-water, had lost 0.052 grs. less than that whereon was fixed a small surface of tin, whilst the tin had lost but 0.001 grs. This trifling loss of the tin must necessarily have taken place in the first moments of immersion in sea-water; whilst the primitive electrical relation between both metals, which they acquired in the atmosphere, was not yet changed. This part of the experiment seems evidently to confirm what was observed by means of galvanometrical researches.

A. VAN BEEK.

UTRECHT, *January* 1837.

On the Geography and Geology of Northern and Central Turkey. Part II. Geology. By Dr A. Boue'. Communicated by the Author. (Concluded from p. 270 of preceding volume.)

European Turkey contains all kinds of *Plutonic erupted masses* which occur elsewhere, with the exception of the old secondary porphyries.

Large erupted masses of *granite* occur in the central parts of Turkey; in the Perindagh; in the Kreshna hill in the Rilo-Planina; in the Despotodagh S. of Philippopolis; W. of Kostendil; at Istip; between the plains of Seres and Salonichi; in the northern part of the Bitoglia chains; in Servia, &c. In this last country I did not find many granites; but Leptinites (whitestone) occur to the S. of the Levanza valley, and there are veins of pegmatite in the gneiss of that place.

To the N. E. of Maidan in the hills of Rudnik, we found a small dome-shaped hill of porphyritic granite among the micaceous greywacke. This granite contains many veins of small-grained granite, which, being harder, are less liable to decompose, and thus form peculiarly shaped elevations. In the Avala hill S. of Belgrade, a dyke of the same kind of granite occurs in transition slates, which are much altered and indurated at the junction, and a bed of compact limestone has been changed into granular.

The granite of the Rilo-Planina and the hills S. of Philippopolis is associated with gneiss, which is often of a very felspathic nature, and with hornblende rocks resembling those in Glencroe in Scotland. The granite has spread itself in the form of veins in all directions through the neighbouring gneiss. These veins are often of the most fantastical forms, and frequently pass into pegmatite granite with garnets. Indeed the granite of these chains seems sometimes to be only large veins or dykes with *salbandes* of granitoidal gneiss.

A most beautiful example of the granitic eruptions is found at three quarters of a league W. of the Convent of Rilo. The gneiss at this place contains a small bed of granular limestone, and the granite has insinuated itself in the form of a dyke through the inclined bed of limestone and the granitoidal gneiss; it even forms a small distinct vein in the limestone, which is partly parallel to the direction of the bed. This gives rise to the most singular juxtaposition of the rocks, and to mixtures of the granitic or felspathic and siliceous matter with the limestone, being nearly of the same kind as the well-known instance in Glentilt, and at Brevig in Norway, between sienite and limestone. The granite is always separated from the white marble by three zones of greater or less thickness and regularity. The first zone or stripe is a granite with green augite, and also sometimes penetrated with calcareous matter; the second is a beautiful mixture of red or yellowish garnet, crystallized or massive, Vesuvian, a little greenish augite, and a large quantity of greyish or bluish quartz or amethyst, as also, although rarely, galena; the third consists of granular limestone, with nodules, veins, or nests of the same mixture of quartz, Vesuvian, and garnet, with fibrous tremolite like that of Glentilt. This last often forms the exterior crust of the nodules of quartz and other minerals. I collected at this place some most singular specimens of rocks, as, for instance, limestone with pegmatite veins, slate with the same and stripes of compact garnet, very beautiful compact rose garnet rock, like that of the Pyrenees, beautiful hornblende and chloritic rocks, granite with epidote and rock-crystal, cavities of calcareous-spar in the limestone next the granite, granite, with nodules of a greenish mineral, probably augite, mixed with black mica, limestone with felspathic spots, siliceous felspathic matter, and the like. The following is a cross section of the rocks from W. to E.: Gneiss of a granitoidal nature, granite, limestone, granite, limestone, garnet rock with pegmatite, gneiss with veins of pegmatite, chloritic and hornblende rocks, slaty argillaceous rocks with veins of granite, gneiss with veins of granite, granitoidal gneiss with the same veins. The rest is covered by debris and forests. The limestone is evidently a bed in the gneiss, for at a quarter of a league to the E. of the Convent of Rilo, the same kind of limestone occurs in gneiss as a bed of fifteen or twenty feet in thickness. In one quarry at this place, the strata, reckoning from below upwards, are as follows: Gneiss with nodules of pegmatite, granular limestone mixed with silica, ten feet of white marble, limestone with tremolite and augite, a hornblende rock with some pyrites, galena, and green copper-ore, compact and crystallized garnet, and lastly, gneiss with very small veins of crystallized garnet. In a second quarry we find, reckoning in the same manner, gneiss, a hornblende or actinolitic radiated fibrous rock, a mixture of this rock with garnets, 10 feet of granular limestone, limestone with tremolite, a bed 1 foot thick of crystallized garnets, 1½ feet of greenish hornblende rock, felspathic gneiss with small stripes of hornblende and garnet; lastly, common gneiss.

In the Perindagh the granite seems to occur oftener in the shape of small dome-shaped hills, as in the undulating plateau of the Kreshna hill; when this happens, the gneiss contains fewer veins of pegmatite. The northern and western sides of the Perindagh are quite covered with these small granitic domes. On the northern declivity of the Kreshna hill, white granular limestone is also seen associated with quartziferous gneiss containing veins of coarse pegmatite, and hornblende rocks. There are two beds of this limestone, and the part in contact with the gneiss contains augite.

Between Seres and Salonichi, particularly between Gumentsche and Schafsha, the decomposed granitic domes are surrounded by gneiss with felspathic or pegmatite veins. The same arrangement is also beautifully seen 1½ leagues W. of Kostendil on the road to Egri-Palanka, and in this case the granite is porphyritic. Many of these veins are nearly parallel to the direction of the slaty structure of the gneiss. I found a most singular intermixture of granite and gneiss with granitic veins in the hills W. and N. of Perlepe, on one of which are situated the ruins of the castle of the famous Servian hero Marco Kralowitsh, the terror of the Turks.

Sienite occurs in Servia in the N. E., central, and S. W. parts; and in Turkey on the Pepentz at two leagues N. of Uskub; on the Strymon near Kosnitza (E. of Kostendil); at Dubnicza; and in the Neretska and Florina-Planina. In central Servia the sienite forms a chain of hills, running W. and S.W. of Kragojevacz, between granular limestone and primary (transition) slates. Similar rocks also occur in the Rudnik hills, near Karanovatz, and in the Kopaunik hills. In this last range the sienite forms a large bed-like dyke in the midst of the slaty greywacke rocks, and the slate at its junction with the igneous rock is converted into *hornfels.* At the summit of the Kopaunik (Kopaunegh), we find, in contact with each other, quartzose gneiss, quartzite, *hornfels,* garnet rock compact or crystallized, slate distinctly altered into a compact jasper-striped rock or *hornfels,* garnet rock with nests of magnetic iron-ore and carbonate of copper, sienite, and gneiss. Beautiful porphyritic sienites are met with on the western and N.W. declivity of the Kopaunik, and farther on there are decomposed varieties. The sienitic dykes run E.—W.; and the chain of hills, and also the slates, run N.—S. The sienite extends from that part of Servia into Bosnia, as in the Ratschka valley in the Novibazar district, where I also observed some altered slates. The sienite in the gneiss of the Florina-Planina contains crystals of sphene and nodules of fibrous black crystallized hornblende. It forms a dyke in the gneiss, in which latter kaolin occurs; at the junction the gneiss is of a whitish colour, with pale greenish mica. In one part of the dyke I observed a patch of quartzose gneiss, which had the appearance of having been partly fused. Higher up, quartzite, and talc-slate with nodules of quartz, are associated with the gneiss, and the sienite is found decaying in the same manner as the slates.

Protogine forms a vast deposit in the wild hills E. and N.E. of Castoria. This beautiful rock is sometimes porphyritic. It is bounded on the E. by gneiss, and on the W. by recent slaty arenaceous limestone rocks and greywacke; and it probably occurs with the talcose conglomerate rocks which form the north side of the Lake of Castoria. The gneiss gradually passes into a series of rocks in which gneiss is subordinate to chloritic and talcose slates, with quartzite and leptinites. These last rocks constitute the whole eastern side of the hills from Bitoglia to the S. of Florina. It was not possible for me to see the junction between the protogine and the preceding rocks; they are separated on the N. by a deep valley. In the Tschardagh it appears that there is not only gneiss, but also granitoidal protogine, as I found blocks of it in the old alluvium north of Kacsanik, but it is by no means so distinct as that of Castoria.

Serpentine is pretty frequently met with in the transition series of Servia. It occurs in veins on the N. E. side of the Avala hill, among the greywacke slates W. of Vratscha (4 leagues from Kragojevacz), where a quartzose siliceous breccia seems to be connected with it; and it is also found farther to the W. before reaching the Rudnik group of hills. It is associated with the same greywacke slate and a fossiliferous limestone at the Castle of Kosnik above the Raschina valley; and it extends into the Uschize district, as well as into the Ratscha valley. But these masses are inconsiderable in comparison to those of the valley of Gratschevatzka-rieka, leading from Bruss to Kopaunik, or rather to a place called Bressetje, where the traveller must sleep the first night, in order to be able to reach the top of the Kopaunik the next day. This small valley presents five most picturesque defiles or passes, four of which are formed by very thick dykes of serpentine and serpentine breccia, contained in greywacke with limestones. Caverns occur in the limestone, and some ores of iron are also met with.

In the upper part of the valley, especially in the part running E.—W. towards Bressetje, I observed the following succession of rocks, viz. slates, serpentine breccia, slaty greyish or reddish limestone running N. E., serpentine, slates, limestone-slates at the village of Radmono, altered slates, euphoditic serpentine breccia, slate, limestone, the same breccia, indurated slate, anthracite slate, yellowish discoloured greywacke, serpentine breccia, a kind of *schaalstein* or brecciated slate with foreign matter, slate, grey and red limestone, hornblendic amygdaloidal breccia, a kind of very felspathic euphotide, indurated slate, limestone breccia, serpentine breccia, sandstone, red indurated slate, slates and greywacke, slaty limestone, slates and greywacke, grey granular limestone. This series, in which the breccia is in the form of dykes, reminds one of some of the valleys in the *Fichtelgebirge*.

Serpentine also occurs associated with transition limestone and conglomerate near the Convent of Detschiani, on the western side of the White Drina basin. Near Lapushnik, to the west of the Upper Mittrovitza, the same rock is associated with red jasper, among slates with a thick bed of coarse quartzose conglomerate. To the E. of the Mittrovitza, the mica-slate, or talc-slate with quartzite, also contains serpentine, as well as quartzose conglomerate and limestone. Nearly the same rocks are met with in the hills E. of Pristina; but the clay-slate is altered, and of a reddish or yellowish colour. As all these beds run N.—S. or N. N. W.—S. S. E., the thickness of this formation must be very considerable. At Pristina they are partly covered by patches of tertiary rocks, such as clayey marl, and quartzose or limestone conglomerate, with some beds of what appeared to be an indurated freshwater marl, which contain pebbles of quartz. I found serpentine associated with micaceous granular limestone, or *cipolino*, when descending from Kacsanik to Uskub, at the distance of 4 leagues from the latter city. It also occurs in the form of dykes in talcose slaty and limestone rocks, near the Lake of Ostrovo in Southern Macedonia; and to the south of this lake the slates seem to be altered at the junction.

Hornblende rocks have been already noticed, particularly when speaking of the Despotodagh. A tolerably large mass of hornblende rock and hornblende slate occurs three leagues N. of Uskub, in the valley of the Pepentz, in felspathic mica-slates and gneiss. I also observed nearly similar rocks in the lower valley of the Strymon, in a gneiss with leptinite S. of Vistritza; and the gneiss seems occasionally to become hornblendic. Hornblende rock is associated with porphyritic sienite in the hills three or four leagues W. of Kostendil; and granite is also found at the same locality. I have already alluded to the localities of the garnet rock, and I may remark, that it is very singular to find garnet rocks so constantly associated with sienite, sienitic porphyry, or granite, in all those narrow beds running nearly N.—S. from the Bannat to the lower part of Romelia.

A *porphyritic and somewhat hornblendic deposit*, like the auriferous rocks in Transylvania, makes its appearance in the Servian hills of Rudnik among the greywacke. These porphyries are sometimes slightly scoriaceous; they are associated with varieties of felspathic breccia, and contain veins which are occasionally very siliceous, and are crossed by smaller veins containing iron-pyrites, copper-pyrites, galena, and hydrate of iron.

Old mines are still to be seen there. Compact limestone occurs on both sides of this porphyry, which constitutes the Malo Sturacz.

A great deposit of *sienitic porphyry* with breccia constitutes both of the steep banks of the Ibar, near Rudnitza; and, as in Hungary, trachytes are not far from it. A much more extensive sienitic porphyry formationo ccupies the whole neighbourhood of Karatova in Macedonia, and is surrounded by mica-slate and talcose rocks, and hills of trachyte and trachytic conglomerate. The porphyry is generally greyish or bluish, very often containing crystals of hornblende, more rarely of augite. It occurs in a decomposed and earthy form in the neighbourhood of the singular town of Karatova, which is situated upon three or four ridges of porphyry, separated from each other by deep and steep ravines. Some brecciated or tufaceous porphyritic masses are associated with it; indeed the porphyry seems occasionally to be in broad dykes, with *salbandes* of breccia containing fragments of slaty rocks, as also of porphyry. An example of this is seen one league to the W. of Karatova, in the valley of Braunitza. Ascending from that town to the south, we leave the decomposed, sterile, greyish-blue porphyry, and meet with unaltered rocks, which establish the connection between the secondary porphyry and some trachytes, or even phonolites. Breccia is also seen here and there, but all the relations of the rocks are obscure in this wild country. All that can be said, is, that masses or hills of porphyry with conical or table-shaped summits are placed next each other.

At 1½ leagues S. W. of Karatova, a mine has been established in some small veins containing argentiferous galena, associated with a little quartz, carbonate of lime, and hydrate of iron. These veins are found in a particular zone of the porphyry, running E.—W.; and the rock is, as in Hungary, siliceous near the vein, and of a whitish or greyish colour. The surface portion of the metalliferous stripe has been excavated in an unskilful manner, and the mines, which are provided with a large pit, are not well managed. In the smelting-house the ores are roasted and smelted in such a way that I observed, in the heaps of scoria, a quantity of galena nearly in its original state.

Perhaps few geologists will agree with me in separating these metalliferous porphyries from the trachytes, because they cannot reconcile themselves to the idea of porphyry of the age of the more recent chalk, or even of the oldest tertiary period. Besides, many geologists consider as trachytes all porphyries with crystals of glassy felspar; now these crystals occur pretty often in my porphyries, but I saw none in my decomposed varieties. The sienitic porphyry in dykes among the slaty primary (transition) rocks of the Bannat also contains crystals of glassy felspar. It is to be hoped that competent judges may soon travel over South Eastern Europe, where trachytes as well as sienitic porphyries occur, and they will soon agree with Beudant and myself in thinking that Montdor, the Cantal, the whole of Italy, the Alps, and the lowest portions of the Rhine, present no porphyritic rocks like those which I have mentioned as occurring in Turkey, and which I regard as somewhat older than the trachytes.

On the other hand, to determine the exact limits of both deposits, would be in most cases, I think, extremely difficult, if not as impossible as to distinguish exactly among a heap of lavas. The trachyte has flowed more frequently as *coulées*. Sienitic porphyry has never yet been seen overlying tertiary rocks; it only occurs in the form of dykes or hills among primary (transition) or crystalline slates.

The trachytic rocks occupy much space in Turkey. The first time we met with these was at Novibazar, in the conical hill on which stand the remains of the Church of Stupani St Georg (the columns of St George). This trachyte, together with some trachytic breccia, extends to the north, and may possibly be in some way connected with a vast deposit of the same kind, which lies between Rudnitza on the Ibar, and Ratschka on the road to Novibazar. The southern part of the Uschize district in Servia is of the same nature. On the road to Novibazar, the trachytic conglomerates present many varieties containing mica and hornblende: they succeed to the compact sienitic porphyries of the Ibar, and occupy much space in that triangular part of country to the south of the confluence of the Ibar and Ratschka.

In crossing between Magoritsch and Strazin in Upper Macedonia, we found an immense range of trachytic hills, with a vast quantity of all kinds of felspathic tertiary conglomerates. The hills with their table-shaped summits shew, from a great distance, their igneous nature; they look like volcanos which have left cones of scoria. These trachytes seem to run N. and S., or N. E. and S. W.; and they extend not only to the north, but also to the south along the river Egridere. They occupy the greatest space on the right side of the river, and only occur overlying, or in juxta-position to, the talcose slates and mica slates on the other side. Indeed the trachyte extends to very near Karatova, and thence at least six or seven leagues to the S. and S. W. of the Breganitza river.

These trachytic deposits consist of trachyte containing mica or hornblende, surrounded by vast masses of trachytic conglomerate, which are well seen between Shinnie and the alluvial basin of Strazin, or between the convent of Dveotatz and the Breganitza; some are whitish, greyish, and sometimes having the appearance of pumice. Semi-opal is found among them, even in the form of a whitish or greyish-black silicified bed, as at the northern entrance of the Egridere among the trachytic rocks. To the west of Karatova, at the distance of one or one and a half leagues from the town; there is also a great quantity of whitish trachytic conglomerate, and a conical hill of hornblendic trachyte is surrounded by it. A singular circumstance is, the association of these conglomerates with the tertiary sandstone and conglomerates, as is seen on the road from Karatova to Strazin, and particularly near the inn called Vukhan (inn of the Wolf). The superposition of the trachytic conglomerate on the talcose (occasionally ferruginous) slates is evident in many places on that road; but tertiary quartzose conglomerates, aggregations of slaty fragments, and molasse, are also observed there in a pretty high situation. In the descent between Vukhan and the valley of Karatova, we can distinctly see, resting on the talc-slate, horizontal tertiary beds composed of quartzose conglomerate with fragments of felspathic rocks, micaceous slaty sandstone, quartzose conglomerate, and molasse. Now these rocks seem to be connected with the molasse and slate conglomerate which occur a little higher up, in beds slightly inclined, and occasionally much penetrated by calcareous matter. These last, indeed, are nothing else than a limestone with fragments of slates, oysters, encrinites, and echinites (as at one-fourth of a league north of Vukhan), and resemble some similar rocks associated with the coralline upper tertiary limestone of Transylvania.

The occurrence of these marine remains at such a height is a peculiar fact in Turkish geology; and future observers should make a point of ascertaining whether the trachytes are not the cause of such an uncommon elevation of tertiary rocks.

In the valley below Karatova I observed a reddish trachytic tufa, or aggregation of fragments of trachyte, resting on the talcose ferruginous slate. Between Karatova and the monastery of Dveotatz, we found similar rocks, in which we observed the globular decomposition of the trachytic conglomerate. The convent itself is surrounded by small hills, the last of the trachytic group, and, as in Hungary, among these is found a very fine porphyritic millstone or buhrstone (*porphyre molaire*). This rock is merely a fine trachytic conglomerate, much indurated by infiltration of siliceous matter. Vast quarries show the extent to which this useful material is exported.

This trachytic group also contains other rocks, viz. doleritic basaltic hills surrounded by tertiary sands and sandstones; near Nagoritsch there occur at least three or four such hills, with steep acclivities, and of inconsiderable elevation.

Another trachyte group fills up the higher part of the Egridere valley, between a place two leagues east of Egri-Palanka, and a valley three leagues east of Kostendil. This large chain, which is covered with woods and meadows, extends also north and south. To the west it contains talc-slate with mica-slate, and to the east gneiss with granitic rocks. A hill elevated at least 1500 feet above the valley through which the road from Egri-Palanka to Kostendil passes, is formed of a decomposed yellowish-grey trachyte with quartz; and the sides of the hills consist chiefly of trachytic conglomerate, which occasionally covers the trachyte as well as the talc-slate. These conglomerates alternate with rocks resembling tertiary marl, even with molasse, and a conglomerate composed of slaty fragments; thus showing the height attained by the tertiary waters, which, had it not been for these trachytic eruptions, and those of Strazin, would have extended in a direct line from the Kostendil basin to that of Uskub. I may remark, however, that I only observed alluvial matter in the Strazin basin, and that, for two leagues to the west of Egri-Palanka, the Egridere is a very small rent running W. S. W. and E. N. E.; and apparently of very recent formation, as no old alluvium is met with.

I think we may consider these trachytes as immediately connected with those around Karatova, or at least as subordinate portions of that great centre of igneous action. Its northern limits cannot be far from the Egridere valley; but I was unable to determine its southern limits, as, in order to do so, I must have crossed from Dubnitza to Karatova by way of Bobosh; a journey of sixteen leagues, in a hilly woody country; however, I shall perhaps ascertain this some other time. Still, although analogy with Hungary may point out Karatova as a centre, I found to the north of Kostendil and Radomir so many recent igneous eruptions, that it will also be necessary to examine whether they are connected with those of Egridere and Karatova. Indeed, on looking from the hills about Radomir towards the west, and especially the north-west, the eye is struck by the appearance of many conical and singularly shaped hills, most of which are pyrogenic, doleritic, or felspathic, and only a few calcareous. Between Radomir and Gerlo I only observed compact limestone and slate, with alluvium or löss; but at Gerlo there occur five doleritic cones, all in a line running N. N. W.—S. S. E. Three of these are situated to the east, and two to the north of Gerlo. The rock is generally a basaltic dolerite with crystals of augite and sometimes of felspar, and a tufa of the same nature, which is sometimes partly amygdaloidal with occasional small veins or druses of zeolite (mesotype). The most curious fact is, that these rocks have made their appearance in the midst of tertiary sandstones and marls, which they have upraised, so that they now incline to the N. E. at an angle of 45°. This is clearly seen in the small hollows east of Gerlo, where we find the highly inclined junction of the doleritic tufa with the indurated and altered tertiary sandstone and marls. The tufa even appears in some cases to be interposed as a kind of *salbande*, or aggregate of the debris of the rocks cut through by the dolerite; in short, it lies between the dolerite and the sandstone. Above the tufa we find distinct alternations of marl and sandstone, and marl and tertiary limestone with oysters, encrinites, echinites, bivalves, and univalves; and lastly, a quartzose sandstone with fragments of Pinna and other organic remains.

At three leagues north of Gerlo, after passing over tertiary molasse, which is superimposed on old compact limestone, we suddenly enter an inconsiderable tract of country covered with small dome-shaped hills of dolerite, or rather of an augitic porphyry-tufa, like that of Southern Tyrol. Red, green, and black are the chief colours of the tufas which occur a league S. and S. S. E. of Niemele, in the Novocelsko Rieka valley.

I observed a very beautiful augite porphyry and tufa still more to the north and west, at a league's distance from Niemele on the road to Scharkoe; and, among the hills on the north and west of the road, some lofty precipices indicate similar deposits. The same rocks, and particularly tufa, constitute a series of hills N. W. of Sharkoe, from within three-fourths of a league of that town to the inn of Czernoklishki-han. The river of Sharkoe has cut its way through these rocks, and the high road passes over the first hill, the river being bordered by precipices.

A great trachytic chain seems to exist in Macedonia, south of Gafadartzi; it may possibly be connected with the chain to the E. of Vodena, and with those trachytic conglomerate hills which form an extensive but low range to the south of Vodena, extending to the east and west of the Vistritza river. In these hills I observed not only trachytic conglomerates, but also pumice conglomerates. The pumice rock borders the marshy plain.

In more Eastern Turkey, trachytic or igneous deposits seem to abound; not only on the Bosphorus, as is well known, but probably also in some small hills not far from Eski-Sagra on the road to Adrianople, as Major Hauslab presumed. True, great basaltic deposits do not exist in European Turkey.

As intimately connected with the igneous rocks, I must mention the great quantity of *hot springs* in that country. They are always in the vicinity of trachytic or sienitic rocks, and are almost all impregnated with sulphuretted hydrogen. They appear to be distributed chiefly in three lines, two of which run N.—S., and the other E.—W. On the N.—S. line, which runs through Eastern Servia, we find Mehadia in the Bannat, Banja near Brestowaz, Banja north of Alexinitza, Sverlik in for two leagues to the west of Egri-Palanka, the Egridere is a very small rent running W. S. W. and E. N. E.; and apparently of very recent formation, as no old alluvium is met with.

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A great trachytic chain seems to exist in Macedonia, south of Gafadartzi; it may possibly be connected with the chain to the E. of Vodena, and with those trachytic conglomerate hills which form an extensive but low range to the south of Vodena, extending to the east and west of the Vistritza river. In these hills I observed not only trachytic conglomerates, but also pumice conglomerates. The pumice rock borders the marshy plain.

In more Eastern Turkey, trachytic or igneous deposits seem to abound; not only on the Bosphorus, as is well known, but probably also in some small hills not far from Eski-Sagra on the road to Adrianople, as Major Hauslab presumed. True, great basaltic deposits do not exist in European Turkey.

As intimately connected with the igneous rocks, I must mention the great quantity of *hot springs* in that country. They are always in the vicinity of trachytic or sienitic rocks, and are almost all impregnated with sulphuretted hydrogen. They appear to be distributed chiefly in three lines, two of which run N.—S., and the other E.—W. On the N.—S. line, which runs through Eastern Servia, we find Mehadia in the Bannat, Banja near Brestowaz, Banja north of Alexinitza, Sverlik tr to separate the iron; and the smelting of it is not less curious; the kiln is opened every sixteen hours, and an immense quantity of charcoal is consumed. The iron is cast in the form of a saddle, in order that it may be the more easily transported on asses. These mines afford a great quantity of iron, which would be of good quality if it were properly treated.

The *general directions of the mineral masses* in Central Turkey are the following:—in the Rilo-Planina N.—S. or N. W. —S. E., the chain running N. W.—S. E. In the Perindagh E.—W., the chain running W. N. W.—E. S. E. The talc-slates to the north of Karatova and in the Egridere run E.—W. At Kacsanik the old slates N.—S. At Pristina, and to the west of that plain, the slates run N.—S, or N. N. W.—S. S. E., which is also the general direction of the primary (transition) rocks throughout all Servia and Mœsia Superior. In the Tschardagh the direction is also N.—S., but the chain runs N. E.—S. W., so that the direction of the chain is different from that of the beds; while, on the contrary, in Servia, Mœsia, and near Pristina, the direction of the chain is conformable to that of the beds. In the Ipek group the beds run N. E.— S. W., and the chain S. E.—N. W., but in the Kurilo-planina the beds run N.—S. The cretaceous beds in the white Drina basin run N. W.—S. E., or N.—S. or N. N. W.—S. S. E. In the greywacke of the Raschina in Servia we observed a direction of N. 20° E. to S. 20° W. In Southern Macedonia the talcose gneiss and dolomite east of Perlepe run E.—W.; the gneiss near Perlepe S. W.—N. E.; the chains running N. W. —S. E., or N. N. W.—S. S. E. The talc-slates near Bitoglia run N.—S., inclining to the east. The gneiss between Bitoglia and Castoria N.—S., and also E.—W., the chain running nearly N.—S. At Castoria the limestone and sandy beds run N. W. —S. E. At the convent of Lisito near the lake of Castoria, the gneiss, and hornblende and actinolite rocks run E.—W. At Klissura the gneiss runs N.—S., inclining to the west. The greyish limestone in the slates between Gafadartzi and Trojak runs N.—S., and inclines to the east conformably with the direction of the chain. The secondary limestone between Sarigöl and Vodena, and the slates in the neighbourhood of the lake of Ostrovo run N.—S. The secondary limestone of S. W.

Servia and Bulgaria runs N.—S., or N. E.—S. W., or N. W. —S. E. From these few data it appears that the nearly N.—S. direction predominates in the central part of the countries described; whilst the N.W —S. E. direction prevails in Western Turkey, as far as the Pindus, and on both sides of the Tschardagh range, which runs N. E.—S. W. In Macedonia and Romelia we again find, to the west, the N.—S. and E.—W. directions, and to the east the same directions.

The conformity or parallelism in the directions of the chain and beds is only found in central Servia, and some other parts, as near Bitoglia, where the direction of the beds is N.—S. as in Servia.

These are the chief facts which I ascertained on my first journey to Turkey; and as it is quite a new country, I have only been able to give a general view of it. In my next journey, however, I intend to examine particular places more in detail. The hilly countries of Turkey are not always sufficiently inhabited or opened up by roads, to render it an easy matter to give a detailed account of their geology; indeed, it would require for this purpose, a stationary geologist, and not a traveller. In conclusion, I may state, that although the leading facts have been clearly ascertained, some of my classifications of transition and secondary rocks may be considered as provisional; and I shall therefore be happy to receive information on the subject from any person who may examine these localities

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*On the Traces of a Vast Ancient Flood.** *(From Poggendorff's Annalen,* 1836.*)*

Sefström has studied with attention the geological phenomenon which is connected with the formation of our *asar* and the rolled blocks existing in them. The long ridges, which we term *asar*, and which consist of accumulations of rounded stones of various sizes, seem to be peculiar to Sweden and Finland, at least they are not mentioned by the geologists of other countries. Such an *as* extends near the north gate to Stockholm itself, where the observatory is built on it; and formerly it reached, under the name of Brunkeberg, down to the Mälar-See, but has in later times been dug down and carried away.—The *asar* are met with in several parts of Sweden, and they are often to be seen, at short intervals, and in the same direction, following one another, for several miles, from north to south, a circumstance which would induce the observer to believe that the separate *asar* are continuations of one and the same ridge.

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* This notice, taken from Berzelius' Jahresbericht No. 16, p. 381, is only a preliminary account of the complete memoir by Prof. Sefström, which is about to appear in the transactions of the Stockholm Academy.

Sefström has remarked that almost in every place where the surface of the primitive rock has been laid bare, and the covering of sand and earth well washed away by water, not only are traces of abrasion to be found, but also innumerable deep furrows or straight channels, which all run parallel to each other, and in a direction from north to south.

This appearance, which hitherto has been but little attended to by geologists, was first examined by Sefström in the vicinity of Fahlun, and afterwards in several parts of Sweden. He has determined, with good instruments, and with the utmost attention to all the circumstances connected with the subject, the direction of these furrows on different parts of one and the same mountain, and on different mountains in the same district. In this manner he has ascertained that these furrows, wherever they occur, are parallel to each other, exactly as if they had been produced by the rubbing of stones across the surface of the mountains. Their direction is in general from north to south, though it is not always constant in different districts, but deviates even on the same mountain a greater or smaller number of degrees, sometimes to the east and sometimes to the west.

When we take a general view of the results of these extensive investigations, they seem to lead us to the following conclusions:—A general flood, which carried with it an immeasurable quantity of larger and smaller stones, of gravel and sand, must have traversed Scandinavia in a direction from north-east to south-west. It rolled along with great rapidity, and during its passage ground off and rounded the north sides of all solid objects, so that we find there no sharp edges and angles remaining; it has formed the furrows by the dragging along of stones over the ridges, and on the east and west declivities of mountains. During its rapid transit, it has thrown these stones in a sort of arch behind the south sides, which latter, therefore, it could not so abrade and furrow; indeed, the south sides have thus preserved their sharp edges and angles, and no furrows are to be discovered on them provided the declivities are not very slightly inclined. Behind such a southern declivity we can perceive the spot where the boulders have been precipitated from the upper portion, and this is recognised by its deeper abrasion. The deviation in the direction of the furrows on the side of the mountain, is always such as must have resulted from the direction of the flood being diverted by the resistance of the mountain, on the east side towards the left, and on the west side towards the right. By a comparison of the direction of the furrows in different places we therefore find that the lofty ridges, the greater, higher, and broader they are, have in proportion diverted the flood round their declivities; but upon the top the flood has always preserved its original direction.— This flood has always shattered in pieces, and transported to great distances, a number of loose fragments of the older and younger formations. Thus we know that large portions have been torn away from the transition formation of West Gothland, and that of these, distinct traces are still met with on the masses of trap which have been erupted from this formation, and have withstood the violence of the flood. Even on these trap rocks Sefström has seen the furrows produced by the stones that have been rubbed along their surfaces.

So far as we can conclude from the observations hitherto made, the masses carried along by the flood must have attained a height of at least 1500 feet, but on mountains higher than 1500 feet there are no traces of furrows. The period of this flood, according to geological reckoning, must evidently have been coeval with that of the Diluvium, or possibly somewhat more recent. But it was anterior to the dispersion of the boulders, that is of the loose blocks of rock which lie scattered in such abundance on the surface of the earth; for these blocks when they occur with *asar* always lie on them and frequently near their crests.

The *asar* were always formed, as it would appear, on the lee side, that is on the south side of the high objects, which last so broke the strength of the stream, as to cause the deposition of the pebbles. It is impossible to conjecture the nature of the force which set the stream in motion. The direction proves, that it was not the rotation of the earth acting on a fluid mass which had not yet acquired the rapidity of rotation.

Sefström thinks that the *riesentöpfe* are a consequence of the action of this stream, and from the time that was necessary for their formation, he concludes that this stream had a very long duration before the equilibrium on the earth was established.—It is not yet known if any thing corresponding to our *asar* and to the furrows on our mountains has been found in other countries; but probably a revolution so violent was not confined to only a small portion of the earth's surface.

Additional Remarks by Mr Poggendorff.

The riesentöpfe (Swedish *Jättegryttor*), which were mentioned even by Bergmann in his "*Weltbeschreibung*" (description of the world) vol. II. 3. 193, are basin-shaped, smooth-sided hollows in solid rock, which are met with at various points in Scandinavia, in the present and ancient beds of rivers, also on the coasts, and on the bed of the sea; and are sometimes so large as to afford room for several persons. Their formation seems originally to have been caused either by a stream of water bursting forth laterally from a projection in a very hard rock, or by one which descended from a considerable height; stones which collected in hollows formed in this manner, and which would be kept by the stream for a long time in perpetual movement, have evidently perfected the work of washing and grinding. From Professor Sefström, to whose oral communication I am indebted for this notice, we may expect a more detailed account of these remarkable appearances.

From the same source I have obtained the information which authorises me to observe that the *asar* always exhibit at their northern extremity, and only there, a fixed, standing rock; a phenomenon which, on the assumption of a violent flood from the north, has led to the conclusion that it was these very rocks, which, by affording shelter from the flood, gave rise to the accumulation of the narrow and far extending alluvial hills. In connection with this remark it is necessary to add, that, according to Professor Sefström, an *as* is seldom more than half a mile in length; and that when it seems longer, it will be found that a new *as* has commenced at the end of the former one, and in a similar direction, from north to south, though not exactly as a prolongation of the first, but somewhat to a side. These remarks, which convey a much more distinct idea of the *asar* than can be obtained from Lyell's account, make it also apparent why *asar* are not every where met with; for they can only exist where the surface presented isolated rocks of great hardness and compactness as a protection against the flood, and as a place for the accumulation of the pebbles transported by it.

On the Condition of Fossil Plants, and on the Process of Petrifaction. By H. R. Göppert.*

The idea of petrifaction has, owing to too great an extension of the signification, been always employed to denote all fossils which had previously been organic beings, whereas it is suited to only a limited number of them. In the coal and coal slates of the older coal and transition formations, we meet with plants carbonized; though it is not always the whole substance of the vegetable we find thus changed, but frequently only its remains, in the form of an easily separated film, or, indeed, only a mere impression. Very rarely we see between the slaty layers the plant, as it were in a dried state, and still perfectly flexible. At this moment I possess two such specimens, which, in Silesia at least, are extremely scarce. One is a seed discovered by Mr Beinert an apothecary at Charlottenbrunn, in the mine of Sophia, which is situated in a coal-formation, containing porphyry, and the other is a new fern, belonging to the genus Alethopteris, from the clay ironstone mines near Kreuzburg, in Upper Silesia, and which was found by Dr Meyer, in a whitish clay, accompanied by *Calamites cannæformis*, *Sigillaria organum*, and *Alethopteris Ottonis*. The seed exhibits under the microscope a perfect *cellular structure*, (more I could not determine); but the fern presents not only the *striped vessels of*

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* From *Poggendorff's Annalen der Physik und Chemie*, vol. xxxviii. 1836.

the nerves of the leaves, the cellular tissue of the parenchyma, the net-like epidermis, but even also stomata, just as we find in the ferns of the present day. On heating it, there remains behind a skeleton composed of potash, similar, according to my observations, to the skeletons left by recent ferns,* in which indeed, the jointed rings of the sporangia are also similarly constituted. A drop of water destroys the whole structure, and dissolves every thing except an exceedingly minute residue of silica. These facts are of importance in a geological point of view, as they prove decisively *that this fern, neither before, after, nor during its envelopment in the clay, could possibly have been exposed to a long-continued aqueous action ; for had such been the case, no trace of so very soluble a salt as potash would have remained.* The investigations of Karsten prove that water, when it acts during the formation of coal, produces this effect; for he found that the ashes of fossil wood and of brown coal contained no trace of fixed alkali.†

If we place recent ferns between soft plates of clay, then dry them in the shade, and gradually expose them to a red heat, we obtain products bearing a striking resemblance to fossil plants. Nay, according to the different degrees of heat employed, we obtain the plants completely fixed in the clay, and varying from the dried brown to the perfectly carbonized condition, but more rarely presenting a shining black appearance; and by continuing the red heat, after the perfect destruction of all organic matter, we obtain an impression, or what was by the older lithologists termed a stone-kernel (steinkern). It is not uninteresting to remark, how the small quantity of carbon, contained in such a plant, can blacken the layers of clay to a considerable distance; from which we may conclude, that the black colour of the slate-clay covering the coal, proceeds, not perhaps from the disintegrated coal mixed with it, but from the plants which it includes. The experiment succeeds still better when we mix powdered coal or asphaltum with the clay. But the impression is always distinguished by a surrounding portion of a different, and for the most part of a darker colour; whence it appears that the carbonaceous matter of the clay, if it is not, as we have supposed, derived from the plant, exercises at least no influence in the conversion of the plant. It is, therefore, by no means the coaly mass, which, as is generally supposed, fills up the space formerly occupied by the plant; but it is the more or less preserved substance of the plant itself, converted into coal, which we find in the impressions. Hence we understand, also, why different species occurring on one and the same slaty layer present different tints of colour and different degrees of lustre; for this is to be attributed to the individuality of the plant. These experiments have succeeded not only with *ferns*, but also with numerous *dicotyledonous plants.* As I have remarked nothing in the plants of the coal-formation, at least in those which I have had an opportunity of examining in Silesia, and in the museums of Berlin and Prague, that would lead to the belief of their having been destroyed by putrefaction, I think we may be entitled to draw the conclusion, *that every thing which we find in this formation presents a true picture of the then existing vegetation, and that nothing has been lost.*

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* The following additional observations have been subsequently published by Mr Göppert: "We can easily convince ourselves of the existence of this skeleton in every plant, by burning the latter at a candle, and afterwards placing it carefully under the microscope. It will be easy to recognise the original structure of the plant before it falls to pieces. This is also possible with extremely thin carbonized sections. Thus, for example, the spiral vessels of the Taxus can be observed without difficulty. It would appear, that it is only owing to the want of proper precautions that we cannot always discover the vegetable structure in coal."

† *Archiv für Bergbau und Hüttenwesen ;* edited by Dr C. J. B. Karsten vol. xii. part 1. Berlin, 1836. "Examination of the Carbonaceous Substances 'e Mineral Kingdom in general; and more especially of the composition e various kinds of coal occurring in the Prussian Monarchy," p. 29.

Lindley placed in water a number of plants, amounting to 173 species, from the most diversified genera, and allowed them to decompose for two years; and he certainly found that the species whose analogues we usually find, or think we find, in the flora of the coal-formation, were the best preserved.* But it is next to be proved if actual traces of destruction have been found, and then it may be time to venture to form an opinion.

When, as already described, we heat the plants included between the plates of clay, until the organic matter is destroyed, we obtain a perfect impression, of the upper as well as of the under side, a condition which may be compared to that in which, at least in Silesia, we find the ferns and other plants in the greywacke, and in the sandstone of the coal-formation; and in the numerous *leaves of dicotyledons* in the quader sandstone, for the last also do not exist here as they generally do, in the form of *petrifactions*, but only as *impressions*.*

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* The Fossil Flora of Great Britain.

Although I have succeeded, by the assistance of fire, in obtaining vegetable products resembling fossils, still, I believe, that their formation in nature has proceeded much more frequently in the moist than in the dry way. I cannot at least understand how otherwise,—(apart from the weighty grounds adduced for this view by Reichenbach)†—we can adequately account for their partial conversion into wood, coal, or stone, all of which often alternate in layers with one another, in one and the same mass. Even in the 16th century, Balthasar Klein and Matthiolus‡ noticed this singular phenomena in a fragment which had been converted partly into coal and partly into *lapis Armeniacus* (probably clay-ironstone). More recently, Professor Link§ has referred to this subject, and expressed the opinion, that the formation of coal had probably taken place in the same manner as the conversion of animal bodies into spermaceti; a view at which Karsten arrived by the way of experiment, in his excellent account of coal (Poggendorff, vol. xii. p. 1., &c.). In order to obtain decisive conclusions on this subject, I have instituted a series of experiments, which, however, cannot afford any result for a long time after I shall have been enabled to return to them, at the termination of my examination of the coals of Silesia.

The vegetation preserved in brown coal often hardly deserves any other term than that of dried vegetable matter; and in fact fossil wood often differs but little externally from wood which has lain for a long period in water.

The idea of petrifaction, then, belongs properly to a comparatively small number of the impressions of wood and stems which we find in all formations, and still more abundantly in boulders at a distance from their original locality, and the term ought always to be so limited. At an early period it was attempted to explain the phenomenon. Agricola* thought that it took place by means of a liquid containing stony matter, which penetrated the spaces of vegetable and animal bodies, and gradually communicated to them a stony character. The later mineralogists, as Scheuchzer, Walch, Schulze, Schröter, Wallerius the elder, agreed in the opinion, that, when a body is petrified, or converted into metal, an exhalation must first proceed from it by which it loses certain particles, in whose room earthy or metallic ones enter, and that thus at last the body is converted into stone or metal. The means by which the exhalation is promoted in animals is, according to the same view, calcination, and in plants the reduction to earth. More recently, so far as is known to me, no one has attempted to trace out this process by an experimental inquiry, it being probably supposed that too long a time would be rerequired for obtaining the desired result. In a lecture † delivered in London by Faraday, at the beginning of 1836, he says, "that we have no knowledge whatsoever of the nature of this process, for the instances of recent fossilization, which have as yet been produced from various places, are mere incrustations of calcareous or even of siliceous matter, where there has been no preservation of organic forms,—none of that beautiful and incomprehensible *substitution* which, while it excites our admiration, baffles our curiosity." For a long time I was occupied in examining the way which nature had employed in this process. First of all, I made the experiment with iron. I introduced plants into a moderately concentrated solution of sulphate of iron, and left them there until the separation of the salt on the outer portions of the plants shewed the sufficient saturation; or I at once soaked smaller portions of plants and sections of wood in the same solution for several days. They were then dried and heated till they no longer suffered alteration of volume, or until every trace of organic matter had disappeared. *On cooling them I found the oxide thus produced in the form of the plant.* I now took thin vertical sections of the *Pinus sylvestris*, treated them in the same manner, and found them so well preserved after heating *that the punctured vessels peculiar to this family were still perceptible. Just as well preserved were the sporangia of Ferns; the pollen,* (*of Arum Dracunculus, Ricinus communis, &c.*) ; *mosses,* (*Hypnum splendens, intricatum, Fontinalis squamosa*) ; *and even Fungi, as Agaricus deliciosus, Clavaria flava, &c.* After these successful experiments, I was desirous to perform others with a solvent of silica. In vain I tried the common solution of silica, (*Kieselflüssigkeit*). For when, after heating, the silica remained in the form of the plants, the mass, as was easily understood, disappeared on being cooled. I obtained a more successful result by dipping in a volatile acid such as the acetic, and before the application of heat, the fragment that had been soaked in a siliceous solution; but still a portion of the silica taken up by the plants was separated, and so irregularly that it became impossible to recognise the structure. *Silico-hydro-fluoric acid*, prepared according to the formula of Berzelius, answered my wishes better, for the fluoric acid was volatilized, and the silica remained in the form of the plant. Similar results were obtained with most of the other earths and metals, and I generally selected combinations whose acids were easily decomposed by heat, as acetate of lime, acetate of magnesia, sulphate of magnesia, which were all converted into carbonates; nitrate of silver, nitrates of gold and platina, which were all converted into reguline metals; acetate of copper, which was converted into brown oxide; acetate of nickel and bichromate of potash into olive-green oxides; acetate of lead into yellow oxide; manganese into metallic shining manganese; cobalt, wolfram and molybdena into oxides; and all of them retaining more or less the vegetable structure. In proportion as the number of vessels in a plant is greater, and therefore the quantity of cellular tissue less, so much the more perfect are the results obtained in these experiments. In very delicate portions an immersion for a few days is sufficient, and in larger pieces a longer period is requisite; but upon this subject I cannot communicate very exact information, as I only discovered these facts a few weeks ago*.

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* I have not yet had an opportunity of examining, at the localities, how the fucoids of the Jura formation occur.

† Poggendorff, vol. xxxi. p. 511.

‡ A. Matthioli *Epistolæ, edit.* Bauhin, vol. iii. p. 142. *Lugduni Bat.* 1564.

§ Travels in Auvergne, by Legrand. Edited, with remarks and additions, by H. F. Link, Göttingen, p. 85.

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* *Lib.* 3, *de ortu et causis subterran.* p. 507; *Lib.* 7, *de natura fossilium*, p. 639.

† The Lancet, February 6. 1836.

In order to ascertain the change undergone by the organs of the plants, I placed the above mentioned products in water.

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* I for the first time made public this discovery on the 6th July (1836) at the meeting of the Natural History Section of the Silesian Society.

The potash skeleton, which may be distinctly seen in most plants,* is dissolved, and I thought at first that I remarked (and I so stated it in the preface to my work on fossil ferns, p. xix.) that only the vessels were filled or injected with metallic or earthy matter, and that their sides were destroyed by the action of the fire. When, however, I experimented in the same way on several plants which contained less alkali, I saw, in company with my much respected friend Mr Purkinje, that, for example, by immersing in an iron solution the wing-like appendages of the seeds of the *Pinus sylvestris*, † *the walls of their peculiar fibre-like formed cells were actually converted into iron ;*

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* The interesting memoir of Struve (*de silicia in plantis nonnullis. Berol.* 1835) treats of the siliceous skeleton of plants, and I had ample opportunity of confirming his results. Several plants, as the *Chara*, yield a calcareous skeleton.

† Regarding the peculiar structure of the still little known membranes of the seed-capsules, Professor Purkinje had, in the year 1832, instituted a series of interesting investigations, of which I am induced to give here a more particular account, because the conclusions at which he has arrived have been only published in a tract not intended for sale, and which has had but little circulation, viz. in the " *Uebersicht der arbeiten und verhandlungen der schlesischen Gesellschaft für vaterl. kultur im J.* 1832 ;" p. 62 to 65. On the 29th November 1832 Professor Purkinje continued the microscopic investigations commenced in the previous year, on the peculiar structure of the inner membrane of the seed capsule, and for this purpose he employed a microscopic collection of 100 specimens, which he demonstrated by lamp light to the numerous members present, and with the assistance of Plöss's large microscope. The continuous union in most plants of this membrane with the epidermis by means of the style, might be expected from the analogy, if not from the identity, of the two membranes ; (just as in the bodies of the higher animals the epidermis, entering into the interior, stands in connection with the mucous and serous membranes). This analogy was still more confirmed by numerous examples which occur in different genera, viz. in the Liliaceæ, Ranunculaceæ, and Solaneæ, where the membrane is frequently perfectly similar to the epidermis, and where even, in the first two, distinct stomata are met with. But this analogy seems somewhat doubtful when we notice that in most genera, as in the Leguminosæ, Siliquosæ, Rosaceæ, &c., the fibrous structure of this membrane predominates so much that it would appear bold to find in it an analogy or identity with the epidermis, as its fibrous tissue would rather point to a resemblance to the under bark. This contradiction disappears when we examine whole ranges of forms, and understand the separate members in their transitions, for then we find that the most opposite extremes can be reconciled. The Liliaceæ are best suited to this kind of investigation, partly because in them the inner capsular membrane is remarkably well developed, and partly because they exhibit the perfect series from the cellular structure to the most

and that, in the case of a vertical section of the Pinus sylvestris, which was soaked in silico-hydro-fluoric acid, the punctured vessels were converted into silica. In those which are changed into reguline metal we see this phenomena very distinctly if we continue the red heat for only half an hour. By a longer continued action of this degree of heat, the arrangement of the metal is so altered that the connection of the vessels and cellular tissue is somewhat broken; and now, (I cannot suppress the remark, although I do not draw any conclusion from it), there is a great resemblance to those hair-like forms in which the above mentioned metals sometimes occur in the native condition. The richer a plant is in potash and cellular tissue, a condition which occurs in herbaceous plants, the less do these experiments succeed. It is true, that after the heating there appears in the form of the plant, the earth or the metal which has been employed, but, on pouring water over it, nearly every thing is dissolved, and only separate vessels or cells remain behind; as, for example, we remark, in the leaves of the ferns. Although these experiments, which also promise much advantage to vegetable physiology, are capable of being carried much farther, yet, when we apply their results, first of all, to the process of petrifaction, we can already understand why *only trees and shrubs occur truly petrified and never herbaceous plants.* Shrubs occur more rarely than trees, because, though they contain less potash than herbaceous plants, yet

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distinct division into fibres. The series of metamorphoses of this structure may be represented in the following manner: The separating walls of the epidermal cells gradually become individually developed, by acquiring more substance, and by predominating as net-work over the cellular intervals. These intervals become more and more contracted by the increase of the parietal substance, and the fibrous tissue then extends itself; at last there appear anastomoses between the thick parallel fibres, which, being separated, exhibit a tendency to form a net, and further indicate their importance as separating walls. The more exact description and classification of these structures will form the subject of future investigations. In the mean time the following chief forms may be enumerated, 1st, cellular membrane; 2d, plain fibrous net with and without stomata; 3d, spiral-curved fibrous net with and without stomata; 4th, moderately stretched fibrous net, loose, dense; 5th, long stretched fibrous net with square anastomoses; 6th, independent long fibres without concrescence, partly straight, partly curved; 7th, long fibres with partial concrescence, and their passage into stone coverings and stone fruit.

when calcined they yield more than trees. If we proceed in this way we shall in future possess in chemistry an important and serviceable assistance for the determination of fossil plants, as we shall be authorized, by the experiments detailed above, to declare with certainty that plants rich in potash can never be petrified; an assumption we are so much the more entitled to adopt, since the experiment with the fossil fern proves, how, in this respect, the vegetation of the ancient world corresponds with the vegetation in the present day. I intend to examine in this manner the most important families of the vegetable kingdom, and I hope, by means of this synthetical method, to attain many desirable conclusions regarding the analogy of many yet doubtful natives of the ancient world.

Animal bodies, as dry fibrous muscles, can also be altered in this manner, but whether they can be converted into another substance I do not venture to assert; but the experiment succeeds with insects, as with flies, gnats (whose more delicate parts, the wings and feelers, are well preserved), the muscles of crabs, and also with infusory animals. Thus I saw quite distinctly a species of Daphnia (from the half putrid water of a water-barrel) which had been placed in a solution of iron, become converted into iron after being exposed to a red heat for half an hour, and even its feet were thus changed. If, then, we were to place infusory animals, whose skeletons did not consist of silica, in a siliceous solution, and then heat them to a red heat, we might be able to form artificially *Bergmehl*, tripoli, and polishing slate, whose composition has recently been unfolded by the extremely important discovery of Ehrenberg. Evidently here also the larger or smaller quantity contained by the animal organs of solid materials insoluble in water (viz. phosphate of lime) would exercise great influence on the success of the experiment. In the parts richly provided with fat, that substance would oppose insuperable obstacles to the preservation of the form, for during the heating it would swell out and change the whole into a formless mass. I intend to prosecute also these experiments; and, in the mean time, we may perhaps regard the last mentioned circumstance as the reason that animals of a higher class cannot be petrified.

The experiments now communicated seem to me to throw an important light on the process of petrifaction. We may assert with safety, that the first act began with impregnation, and that then the organic matter was removed either by a high temperature, or by the moist method, or by a gradual decay. The last seems to me by much the most probable, and hence also the greater compactness of fossil wood may be explained, a topic which, owing to the extensive range of the whole subject, and the short time devoted to it, I did not reach. Although nature certainly did not employ the acids which I used, in her formation of the woods converted into flint or calcedony, yet the possibility of imitation has here been proved, and we may hope that yet further elucidation of the subject may be attained by other means.[*] But, before succeeding, I will not speak of the attempts which I have already commenced to reach the desired object. In conclusion, I have to remark, that specimens have been sent of the imitations of organic remains to the collections of Berlin and Breslau.

On the Meteoric Showers of November 1836. By Denison Olmsted, Professor of Natural Philosophy and Astronomy in Yale College.[†]

For six years in succession, there has been observed, on or about the 13th of November of each year, a remarkable exhibition of *shooting stars*, which has received the name of the " Meteoric Shower."

In 1831 the phenomenon was observed in the State of Ohio,[‡] and in the Mediterranean, off the coast of Spain.[||] In 1832, the shower appeared in a more imposing form, and was seen at Mocha, in Arabia ;[§] in the middle of the Atlantic Ocean ;[¶] and near Orenburg, in Russia ;[**] and at Pernambuco, in South America.[††] The magnificent meteoric shower of 1833, is too well known to require the recital of any particulars. Of the recurrence of the phenomenon at the corresponding period in 1834, and in 1835, evidence has been presented to the public in previous numbers of this Journal. (See vols. xxvii. pp. 339, and 417.; xxix. 168.) I now feel authorized to assert, that *the meteoric shower reappeared on the morning of the* 13*th November* 1836.

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[*] In a more recently published notice, Mr Göppert remarks, "By placing plants for some time in silico-hydro-fluoric acid, I succeeded in obtaining a coating of calcedony, which was perfectly clear and transparent, and resembled hyalite. I made this observation at the beginning of August, and shewed the result of the experiment to several friends.

[†] Silliman's Journal, vol. xxxi. p. 386.

[‡] Amer. Journal of Science, vol. xxviii. p. 419.

[||] Bibliotheque Universelle, Sept. 1835.

[§] Amer. Journ. xxvi p. 136.

[¶] Edin. New Phil. Journ. *July* 1836.

[**] Ibid. 349.

[††] New York American, Nov. 15. 1836.

It has been supposed by some, that the appearance of an extraordinary number of shooting stars, at several anniversaries since the great phenomenon of November 1833, can be accounted for by the fact, that so general an expectation of such an event has been excited, and that so many persons have been on the watch for it. Having, however, been much in the habit of observing phenomena of this kind, I can truly say, that those exhibitions of shooting stars which have for several years occurred on the 13th or 14th of November, are characterized by several peculiarities, which clearly distinguish them from ordinary shooting stars. Such peculiarities are the following:

1. The *number of meteors*, though exceedingly variable, is much greater than usual, especially of the larger and brighter kinds.
2. An uncommonly large proportion leave *luminous* trains.
3. The meteors, with few exceptions, all appear to *proceed from a common centre*, the position of which has been uniformly in nearly the same point in the heavens, viz. in some part of the constellation Leo.
4. The principal exhibition has at all times, and at all places, occurred between midnight and sunrise, and the *maximum from three* to four o'clock.

In all these particulars, the meteoric showers of 1834, 5 and 6, have resembled that of 1833; while no person, so far as I have heard, has observed the same combination of circumstances on any other occasion within the same period. I have not supposed it necessary, in order to establish the identity of these later meteoric showers with that of 1833, that they should be of the same magnitude with that. A small eclipse I have considered a phenomenon of the same kind, with a large one; and, conformably to this analogy, I have regarded an eclipse of the sun, first exhibiting itself as a slight indentation of the solar limb, but increasing in magnitude at every recurrence, until it becomes total, and afterwards, at each return, but partially covering the solar disk, until the moon passes quite clear of the sun,—as affording no bad illustration of what probably takes place in regard to these meteoric showers. The fact, that the Aurora Borealis appears unusually frequent and magnificent for a few successive years, and then for a long time is scarcely seen at all, was proved by Mairan a hundred years ago.* There is much reason to suspect a like periodical character in the phenomenon in question, which first arrested attention in 1831, became more remarkable in 1832, arrived at its maximum in 1833, and has since grown less and less at each annual return. Some seem to suppose, that we are now warranted in expecting a similar exhibition of meteors on the morning of every future anniversary; and this, I think, is not to be expected. It is perhaps more probable, that its recurrence, unless in a very diminished degree, will scarcely be witnessed again by the present generation. The shower, however, at its late return, was more striking than I had anticipated; and it must be acknowledged, to be adventurous, to enter the region of predication respecting the future exhibitions of a phenomenon, both whose origin and whose laws we so imperfectly understand.

But it is time to present the reader with the evidence of the return of the meteoric shower on the late anniversary.

Accounts of observations before us shew, that the meteoric shower was seen in most of the Atlantic States, from Maine to South Carolina. We will begin on the north.

I. *Observations made at Springvale Maine. Extract of a Letter from Samuel Dunster, Esq., Agent of the Franklin Manufacturing Company.*

"I requested the watchman at our manufacturing establishment to call me if anything of interest occurred. He accordingly called me at about a quarter before three o'clock, on the morning of the 13th November. At three o'clock I began to count the meteors, and numbered as follows—

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* Traité Phys. et Hist. de l'Aurore Boreale. Par. M. de Mairan. Memoirs of the Royal Academy of Sciences for 1731.

Time.	Number.
3 h. 30 m.	37
3 h. 45 m.	25
4 h.	31
4 h. 15 m.	25
4 h. 30 m.	22
4 h. 45 m.	28
5 h.	22
5 h. 15 m.	16
5 h. 30 m.	20
5 h. 45 m.	11
6 h.	11
6 h. 15 m.	5
	253

The meteors, with the exception of five or six, all had a direction from a point in the eastern part of the heavens, about 15 degrees N.N.E. of the planet Jupiter; and, although they appeared in all parts of the sky, still, if the lines of motion had been continued backwards, they would all have terminated in that point. Having witnessed the meteoric shower of 1833 in Pennsylvania, I was particular to observe the foregoing fact. The phenomenon appeared to me to be identical with that, but far less magnificent. The day preceding had been remarkably rainy, but the night was clear and still.

"Between four and five o'clock, an *auroral arch* was to be seen in the north, and *streamers* at half-past five."

II. *Observations at Cambridge, Mass.; published in the Boston Courier, Nov.* 14.

"At eighteen minutes before four o'clock, a large meteor darted from the north. It was quite luminous, and in size apparently equal to half the full moon. This was succeeded by many smaller meteors, and twenty-three were counted by me during an hour and a half; several were seen by other persons *in the room*,* which escaped my notice. During this time one was observed of great brilliancy, having a luminous train apparently a yard in length. The lightning† continued the whole time, and there was considerable appearance of aurora borealis. —W."—*Cambridge, Nov.* 13*th.*

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[*] From this expression it is inferred, that the writer had but a small portion of the firmament in view.

[†] From light clouds in the south-east.

III. *Observations at Yale College.*

"The preceding day had been rainy, and early the same night the sky was overcast; but before midnight, the firmament became cloudless, and the stars shone with uncommon brilliancy.

"My expectation of a repetition of the meteoric shower at this place was so slight, that I had made little preparation for observing the heavens, although I looked out frequently after midnight. About half past three o'clock, finding that the meteors began to appear in unusual numbers, I directed my attention towards the eastern part of the heavens, whence they appeared mostly to proceed, and closely watched the stars from the Great Bear on the north, to Canis Major on the south, embracing in my field of view about one third of the firmament.

"It was soon discovered that nearly all the meteors shot in directions, which, on being traced back, met in one and the same point near the eye of Leo. For a quarter of an hour from half-past three o'clock, I counted twenty-two meteors, of which all but three emanated from the above radiant point. Ten left luminous trains, twelve were without trains; and the three that did not conform to the general direction, moved perceptibly slower than the others. The greatest part shot off to the right and left of the radiant, the majority tending south towards the Heart of Hydra. The next fifteen minutes afforded but seven meteors, and the number gradually declined until daylight.

"The exact position of the radiant was near a small star forming the apex of a triangle with the two bright stars in the face of Leo, having a right ascension of 145°, and declination of 25 degrees.* Its place, therefore, was very nearly the same as in 1834, differing only half a degree in right ascension; and all the phenomena very much resembled those observed that year, except that they were on a scale somewhat inferior.

IV. *Observations at New York. From the New York American of Nov. 15th.*

"The annual recurrence of this phenomenon being a subject of much interest, the undersigned kept a careful watch on the night of Saturday and morning of Sunday last, and is gratified in being able to announce the reappearance of this phenomenon with considerable brilliancy.

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* This position of the "radiant," as observed here in 1833, was in R. A. 150°, Dec. 20°; in 1834, R. A. 144° 30′, Dec. 30° 15′.

"During the evening, but few meteors were observed, but from eight o'clock until near the dawn, successive flashes were observed in the east, supposed by some to be lightning. At eight o'clock, a very beautiful auroral light was seen of a pinkish colour. This continued for a short time only, although a general luminous appearance in the north remained during the night.

"About two o'clock in the morning several meteors were seen to dart across the Great Bear, and from this time constant watch was kept up until day-light. From two to three o'clock, ninety-eight meteors were counted, some being very small, but the greater number of great size and brilliancy, resembling a rocket both in the explosion and trail left behind,—the trails lasting in some instances for nearly two minutes.

"With two or three exceptions, the course of the meteors was divergent from a point in Leo, declination 20°, right ascension 150°, nearly. The place of this point was fully confirmed during the night.

"From three to four o'clock, 150 meteors were counted, and 300 in all were enumerated. After this time we kept no account of the number, though many more appeared. From the situation of the observer, it is probable that more than half escaped notice. Several were seen in the clear light of the dawn; and Jupiter, Venus, and Mars, all shining with great brilliancy, were alternately outshone by these transient rivals. No doubt now exists in the mind of the writer, as to the distinct and peculiar character of the phenomenon; for though an attentive observer of such matters, he has never seen any thing bearing the slightest resemblance to this display, except on the night of November 12–13. 1832, when he had the good fortune to observe the same appearance while at sea, off the harbour of Pernambuco, one year before the far-famed shower of 1833.

"G. O. S."

V. *Observations at Newark, New Jersey. From the Newark Daily Advertiser.*

This account much resembles the foregoing, as might be expected from the proximity of the two places of observation. The writer remarks that, previous to two o'clock, a few shooting stars were seen, but no more than on ordinary occasions. After that, however, there was a decided increase. In an hour and a-half he counted about seventy-five, although his field of view took in only sixty degrees. After four o'clock, their succession was less frequent, and they continued to diminish in number until the dawn of day. He thinks the whole number that fell was not less than 400.

VI. *Observations at Randolph Macon College, Virginia. By Professor R. Tolefree. Communicated in a Letter to the writer of this Article.*

"On the night of the 12–13th November, three of the students and myself prepared to watch all night. The sky was serene and all was calm. About ten o'clock, meteors began to appear. The first, distinguished for its brilliancy, started from the lower part of the Little Bear, and proceeded to the south-west. After midnight, until two o'clock, all the meteors shot westward, and from two o'clock until day-break, their course was entirely north-west. We only watched occasionally during the night, and only on the northern side of the heavens, except an occasional visit to the other parts of the building.*

"I counted 248 shooting-stars, and my companions saw a larger number than this. You may safely conclude that 500 were seen by us, and this from observations kept up only at intervals during the night."

VII. *Observations made in South Carolina.*—From the *Charleston Courier of Nov.* 25.

"*Greenville, Nov.* 19.—We learn that the people in the neighbourhood of Maybinton, Newbury district, witnessed the fall of an immense number of meteors, which first made their appearance about twelve o'clock on Saturday night last, and continued their descent until daylight next morning. It is said their number was not near so great as that of the "falling stars" three years since, but the spectacle is represented as having been very brilliant and unusual."

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* Had Professor Tolefree taken his station where his view of the firmament would have been unobstructed, he would probably have seen a still greater number shooting to the south-west.

From the foregoing accounts compared, we are led to conclude that the meteoric shower increased in intensity from north to south, that of South Carolina having been the most considerable of all, so far as accounts have reached us.

Does not the recurrence of this phenomenon for six successive years, at the *same period of the year*, plainly shew its connection with the progress of the earth in its orbit? and does not the fact, that the greatest display occurs every where in places differing widely in longitude at the *same hour of the day*, as plainly indicate its connection with the motion of the earth on its axis? The supposition of a body in space, consisting of an immense collection of meteors stretching across the earth's orbit obliquely, so that the earth passes under it in its annual progress, while places on its surface lying westward of each other are successively brought, by the diurnal revolution, to the point of nearest approach, will satisfy both these conditions. I can think of no other that will. The "point of nearest approach" may be merely the extremity, or the *skirt* of the nebulous body; while the greatest part of it, and, consequently, its centre of gravity, lies too distant from the earth to be much influenced by its gravity. It would not be at all inconsistent with the known extent of astronomical bodies, to give to the body in question a breadth of thousands, and a length of millions of miles. It was an accidental observation, made after the conclusion was formed, which ascribes the origin of meteoric showers to a revolving nebulous body, that first led me to suspect the *zodiacal light* to be the body in question. This, according to Laplace, *is* such a nebulous body, revolving round the sun in the plane of the solar equator.*

We actually observe it to reach over the orbit of the earth, making an angle with its plane of only seven and a quarter degrees. It is not difficult to place it in such a situation, that the earth shall come very near to the *skirts* of it at least. We should, indeed, expect this meeting of the two bodies to take place at the nodes of the solar equator, and therefore in December and June instead of November and April. It is easily conceivable, however, that the aphelion of the zodiacal light, at which place it approaches nearest to the earth, does not lie exactly at the node, but so far from it that the earth passes it a month before it comes to its node, at which time, moreover, the earth is more than a million of miles nearer to the sun than its mean distance. In endeavouring to fix the *periodic time* of the meteoric body, since it must be either a year or half a year, (for no other periodic time could bring the two bodies together at intervals of a year,*) several considerations induced the belief, that *half a year* was the true period, an inference drawn especially from the apparent great excess of velocity of the earth at the point of concourse; but the period of *a year* (or, more probably, a little less than a year), by implying that the two bodies are always comparatively near to each other, would better explain the occurrence of shooting stars at all seasons of the year, and would be particularly favourable to the explanation of those meteoric showers which have on two occasions at least,† occurred near the last of April, a time distant about half a year from November, and therefore sustaining a like relation to the opposite point of its orbit. In such a case, meteoric showers would occur in April and November, for the same reason that the transits of Mercury take place in May and November exclusively. The greater frequency of meteors in November than in April, naturally results from the greater proximity of the earth to the sun at the former than at the latter period; to which, perhaps, may be added the effect of the eccentricity of the orbit of the meteoric body, the aphelion being on the side of November. In the present state of our knowledge on this subject, I regard it as a point open for inquiry, whether it will best accord with all the phenomena of shooting stars, to give to the meteoric body a period of nearly one year, or of half a year.

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* Mec. Celeste (Bowditch), vol. ii. 525.

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* See vol. xxvi. p. 166, of this Journal.

† In Virginia, and various other parts of the United States, in 1803, and in France in 1095, making suitable allowances for the more rapid progress of the earth through the winter signs, and for the change of style, and the meteoric shower of the 20th of April 1095, occurred at very nearly the very opposite point of the earth's orbit.

I have been somewhat disappointed that the astronomers should have paid so little attention to the remarkable changes which take place in the zodiacal light about the 13th of November, as has been repeatedly mentioned in this Journal. It appears to me a fact deserving their attention, that the zodiacal light, which for weeks before the 13th of November appears in the *morning* sky, with a western elongation of from 60 to 90 degrees from the sun (while up to that time not a glimpse of it can be caught in the evening sky), should immediately afterwards appear after the evening twilight in the west, and rapidly rise through the constellations, Capricornus and Aquarius, to an elongation of more than 90 degrees eastward of the sun, while it as *rapidly* withdraws itself from the morning sky, and within a few days vanishes entirely from the western side of the sun. For three years past I have observed these changes with much interest, and feel warranted in asserting that they have been repeated with uniform regularity. The present year the light was very feeble in the morning sky, an effect partly owing to the presence and peculiar splendour of the planet Venus; but as soon after the 13th of November as the absence of the moon would permit observations, the light appeared in the west immediately after twilight, crossing the Milky Way, and rising in a pyramid almost as bright as that, the triangular space between it and the Galaxy, embracing the Dolphin, appearing by contrast strikingly darker.

I can account for this great and rapid change of place in the zodiacal light, a change which is unlike any it sustains at any other period of the year, only by supposing that on or about the 13th of November it comes very near to us, and that we pass rapidly by it, thus giving it a great parallactic motion, an effect which is in perfect accordance with all our previous conclusions.

According to this view of the subject, *the zodiacal light would no longer be regarded as a portion of the sun's atmosphere, but as a nebulous or cometary body, revolving round the sun within the earth's orbit, nearly in the plane of the solar equator, approaching at times very near to the earth, and having a periodic time of either one year, or half a year, nearly.*

Such, I affirm, would be the fact should the zodiacal light be proved to be the body which affords the meteoric showers.

Yale *College, Dec. 19. 1836.*

On Unity of Function in Organized Beings. By WILLIAM B. CARPENTER, M. R. C. S., Senior President of the Royal Medical Society, and President of the Royal Physical Society, Edinburgh.* Communicated by the Author.

THERE are few things more interesting to those who feel pleasure in watching the extraordinary advancement of almost every department of knowledge at the present time, than the rapid progress of philosophical views in sciences which have hitherto been too much confined to mere observation. The insulated facts which have been gradually collected by various labourers in the vast fields of comparative anatomy and physiology, are now made the basis of generalisations alike important from their extensive range, and interesting from the unexpected nature of the results to which they frequently lead; and though the application of the laws thus obtained may sometimes appear forced, and inconsistent with the usual simplicity of nature, further investigation will generally shew that the difficulty is more apparent than real (frequently arising solely from our own prejudices), and that it is in many cases the result of that combination of unity and variety by which is produced the endless diversity and yet harmony of forms so remarkable in the animated world.

The object of the present essay, which has been partly suggested by Dr Barry's valuable papers in the last two numbers of this Journal, is to carry out to particulars some of the general principles there laid down, with the addition of others which had previously suggested themselves to me. It is far from my present intention to enter into a critical examination of those papers, more especially as they have in view the laudable object of exciting the attention of English physiologists to a branch of study which has by no means received from them that consideration which its importance demands.

The time has long gone by when similarity in function and external form were considered sufficient for the recognition of analogies between organs; anatomists are now aware of the necessity of resting their comparison upon the elementary structure of organs, their connections with each other, and the changes they undergo during the progress of their development. Neither of these grounds of judgment can, I think, safely be trusted to alone; whilst, combined with each other, they furnish a body of evidence which is quite irresistible. The truth of these observations will, I trust, appear in the sequel; but I might, in the mean time, adduce in illustration the mode in which the true character of the wings of insects is to be ascertained.

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* The above essay was read as a communication to the Royal Medical Society, 14th April 1837.

If we pass in review the various means by which the locomotive organs of different classes of animals have been placed in relation with the resisting or impelling powers of the atmosphere, we shall observe a community of function, and a general similarity of external form, concealing a total difference of internal structure. In most cases, however, we may remark that the wing or other organ of propulsion, however it be constructed, is only a variation from the usual form of a corresponding part in the neighbouring groups; since " nature, in effecting a new purpose, is inclined to resort to the modification of structures already established as constituent parts of the frame, in preference to creating new organs, or such as have no prototype in the model of its formation." The question, therefore, naturally arises with regard to the wings of insects, whether they are to be considered as new organs, superadded to those which are found in the adjoining classes, and in the early stages of their own existence, or whether they can be shewn to be the result of an extension or increase of development on the part of some structure already present, although perhaps assuming a different form. We shall then first inquire what inference may be drawn respecting the real nature of the wings of insects from their anatomical structure. They may be readily shewn to consist of a fold of external membrane, extended upon ribs or nerves, which are principally formed of *tracheæ* connected with those in the interior of the body. It is only recently that the circulation of fluid has been observed in the wings. Carus describes it as visible in the pupa of many species, but he does not seem to have detected it in more than a few cases after the last metamorphosis. My friend Mr Tyrrell of Exeter, informed me, however, about two years ago, that he had witnessed it in many perfect insects, especially the common house-fly, if examined sufficiently soon after its emersion; and I have since had several opportunities of confirming his observations, which have, I believe, been presented to the Royal Society. The truth appears to me to be, that the circulation goes on as long as the wing continues to grow, but ceases when it has arrived at its full size; and this view coincides with the fact that slight injuries of the wing are not repaired in adult insects. The question has been much agitated, whether the circulation takes place in distinct vessels, or whether the fluid passes along the interstices of the membranes forming the wings. Analogies would certainly lead to the belief that distinct vessels exist; and it is easy to explain the difficulty of detecting them in the wing after it has become dry, from the fact that when no longer distended by fluid, they collapse and become shrivelled, so that a transverse section of a rib shews only one canal, that of the trachea, which is kept open by its elastic spiral filament. Mr A. Pritchard of London, pointed out to me a few months ago, however, a wing in which three tubes were distinctly visible in each rib. This structure is exactly analogous to that which exists in the gills of aquatic insects, and hence Oken, followed by Blainville, termed the wings *aërial gills*, an idea which, however ridiculed by succeeding writers on entomology, will, I think, ultimately appear to be supported by the strictest analogy in structure, situation, and development.

The branchiæ of water insects plainly resemble the wings in being composed of expansions of the tegumentary membrane spread out upon nervures formed by tracheæ and vessels. Sometimes the membrane is continuous, so that the gills assume a foliaceous appearance, like that of the wings, but in other cases it is divided, so that the branchiæ more resemble the filamentous tufts of the nereis. The elementary structure is the same, however, in both cases. The position of the branchiæ is constantly varying; sometimes they are attached to the thorax, sometimes to the abdomen, but in every case they have an important relation with the movements of the animal, and are frequently the sole organs of progression with which it is furnished.

From the *structure* of the wings, and their correspondence with the acknowledged respiratory organs of aquatic insects, we might infer their nature with considerable probability; we shall next briefly trace their *connections* in search of the same object. If we cast a glance at the gradual development of the organs of locomotion formed by appendices to the trunk in ascending the scale in the articulata, we shall see their first appearance in the setæ of the earth-worm, and the filamentous tufts of the nereis, the latter serving both as branchiæ and as instruments of progression. In the higher annelides, one of the setæ of each tuft is more developed than the rest, forming a long tubular-jointed appendix to each segment, which is evidently the rudiment of the leg perfected in the myriapodes. The twelve segments forming the body of the caterpillar (which may be regarded as for the time an annelide), are each provided with a pair of legs;* and these are sufficient to execute the movements which the animal requires in this stage of its existence, when the whole energies seem as it were concentrated on the nutritive system. When the adult insect emerges from the chrysalis, however, after losing for a time all appearance of external members, it is found that the nine posterior segments forming the abdomen are entirely destitute of appendages, whilst the three thoracic segments are provided not only with three pairs of legs, but with two pairs of wings attached to the second and third segments. If these wings had taken the place of the legs which disappear during the metamorphosis, there might have been some ground for considering them analogous organs; but if we contrast their position on segments which retain their legs with that of the branchial tufts of the annelides, it must, I think, be acknowledged that we thus derive from their *connections* another strong argument in favour of the view I am advocating.

It remains for us now to consider their development; and though I regard this as an important link in the chain of evidence, I cannot see that it affords more than a corroborative proof, or that we should be entitled to take up such a bold position without a firmer foundation. After the third moult, the rudiments of wings may usually be traced in the caterpillar, assuming the form of laminæ of mucous tissue, and permeated by tracheæ;* and during the chrysalis state, their development proceeds gradually towards the form which they ultimately assume. Now, as the tracheæ permeate not only the wings but the whole bodies of insects, it is evident that this circumstance of itself assists us little; the development of the wings of some aquatic insects, however, affords us more valuable testimony. "As long as the insect dwells in the water, its rudimental wings are true water gills; but so soon as it has quitted the water, they transform themselves into air gills; for, in *both* cases, fluids circulate in their vessels, which doubtlessly receive oxygen from the air." †

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* I am of course speaking in this, as in other cases, of the regularity or typical form.

To enter into all the arguments by which this position might be supported, and to refute the objections which may be urged against it, would lead me too far away from my present object; but I may observe, that it is only by taking an extensive view of comparative structure that we can have any hope of arriving at accurate results; and great care is necessary to dismiss from our minds all prejudice in favour of any particular mode of organization as a standard or type of the rest. Let us suppose an entomologist to form his views of the structure of animals in general from that of the articulata; he would expect to find the wing of a bat or bird constructed on the model of that of an insect; and yet he would not be acting more absurdly in maintaining that this organ is an appendage to the respiratory system in vertebrated animals (especially considering its remarkable connection with this system in birds) than many entomologists in being led by their previous acquaintance with other types of structure, to consider the wing of an insect as a modification of its leg.

In speaking of the separation existing between different groups of organized beings, it is to be recollected that the minor variations from a particular type, whether that be of a class, order, genus or species, are frequently of such a character as to approximate some of its divisions to neighbouring groups; and that, sometimes, the minor or secondary character may become so predominant as to leave us in doubt to which group any individual belongs. I might refer to the characters of the mollusca, so strongly marked in the cirrhopodes, although the latter group unquestionably belongs to the articulated series; or to the characters of the bat and bird, so strikingly displayed in the pterodactylus. Without wishing to enter into the discussion of the circular and quinarian theories, I may state my conviction that Messrs Macleay, Fries and Swainson, are perfectly correct in maintaining, that the types of each group are definitely separated from one another; but that their aberrant members (where the chain is complete) have the strongest relations of affinity. Every one must perceive that the extended researches which are at present being carried on, both in zoology and botany (and these not confined to the existing epoch, but extending to past ages) are every day contributing to fill up the links that before seemed deficient; and it is now generally regarded as the true character of a complete natural group, that it passes by almost imperceptible gradations into every adjoining one. To take the example of the cephalopodes and fishes. Although the former are universally regarded as the most developed of the mollusca, no conchologist would assume the class as the type which most prominently represented the peculiar characters of that division of the animal kingdom. In like manner, fishes, which are the least developed of the vertebrata, are far from being typical of their division. We might expect, therefore, on the principles just laid down, that the hiatus should not be very wide between these two classes; and although Cuvier was of opinion, that an impassable gulf separates the vertebrata and invertebrata, more extended research has shown, that though there may be little general resemblance in form between any fish and any cephalopod, yet there is a very gradual transition in the structure of most of the systems of these two classes. Thus the nervous system, and internal skeleton of the highest cephalopodes, may almost be placed on a level with those of the lowest cartilaginous fishes; the arrangement of their circulating apparatus is strikingly intermediate between that of the mollusca in general and that of fishes; and whilst, in their organs of locomotion, we see a beautiful adumbration of those which are characteristic of fishes, so, in many fishes we may trace the remains of those usually regarded as peculiar to the cephalopodes.* No inferior group of mollusca presents such remarkable approximations to the class of fishes in any stage of development; and in none of them do we observe that symmetrical form and elongation of the trunk which is so prominent a feature in the structure of the naked cephalopodes.

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* Burmeister's Entomology, p. 426.

† Ibid. 442.

We find among the classes which make up the sub-kingdom radiata, a still greater tendency to pass into one another; so that it is almost impossible to fix with precision the limits to each; and every botanist is aware, that however definitely even the primary divisions of the vegetable kingdom may be formed, many obstinate transgressors of their boundaries will be met with, which exhibit a very troublesome fondness for their neighbours' domains.

Dr Barry has quoted from Burmeister the very ingenious remark, that the osteozoa (vertebrata) unite in themselves the development of the nutritive system, which is characteristic of the gastrozoa (mollusca) and the locomotive apparatus of the arthrozoa (articulata). This is a beautiful confirmation of the arrangement of the invertebrata, suggested by Lamarck, who regarded the mollusca and articulata as forming two parallel lines commencing with the radiata below, and terminating in the vertebrata above; each has its own characters of elevation and degradation, and neither can be considered as in every respect superior to the other. It appears to me, that, in the nervous system of the vertebrata, we may trace the combined characteristics of those of the mollusca and articulata. In the former we find a circle of ganglia around the œsophagus, specially connected with the organs of sense, and therefore with the function of nutrition; and in their higher species, these ganglia are almost entirely supra-œsophageal, and thus pass into the cerebral ganglia of the vertebrata, whose spinal cord on the other hand (which is now generally regarded as in itself an originator of power, if not also a seat of sensation), being specially connected with the locomotive organs, is a fair representation of the double nervous column possessed by the typical articulata. Whilst, therefore, this system, being necessarily connected with all the other organs of the body, unites in the vertebrata the types which it presents in the other two great divisions of the animal kingdom where it is distinctly marked, each of the other systems of the vertebrata is, I think, developed upon a single uniform plan. Thus we should not be led to look in insects with any analogies with their nutritive system, nor among the mollusca for any representation of their locomotive organs. The whole structure of the typical mollusca is devoted to the perfection of their nutritive system, and we consequently find, an asymmetrical development prevailing throughout, involving (except in the highest cephalopodes) even their organs of locomotion; and we fully recognize this asymmetrical form in the structure of the thoracic and abdominal viscera of the vertebrata in general. In the articulata, on the other hand, where the functions of animal life so greatly predominate, symmetrical development of the organs of locomotion is the prevailing character; and the form of the nutritive system is made partly to yield to this. This symmetrical development is everywhere characteristic of the organs of animal life in the vertebrata; and the resemblance forcibly occurs to us between the subdivisions of these organs in the vertebrata and the higher articulata, keeping, however, this great principle in view, that in the former, the organs of support are in part of the *neuro-skeleton*, whilst in the latter these are formed by the *dermo-skeleton*. It appears to me, therefore, that in the study of each division of the animal kingdom, we shall find parts analogous to the rest, and that the sum-total of the effect is produced by the proportional development of each system, which would seem, therefore, finally resolvable into a question of degree only.

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* Cyclopædia of Anatomy, vol. i. p. 525.

It by no means follows, however, from this doctrine, that the whole animal kingdom is formed upon the same type, and progressively developed in such a manner that the transitory states of the higher animals furnish exact representations of the permanent forms of the lower. What is meant to be maintained is, that each organ in the progress of its evolution presents analogies in elementary structure and in degree of development (by no means necessarily in external form) with the permanent states of the same in the classes beneath; and this is again to be understood with the limitation just now expressed, which will prevent us from seeking in insects any forms analogous to the nutritive system in the Vertebrata, or from looking in the Mollusca for any representation of their locomotive apparatus. Moreover, it usually happens that the development of the different systems does by no means proceed *pari passu*, and hence the embryo cannot be considered as presenting in its totality any resemblance or analogy with beings beneath it; and it is deficient in this very important faculty, the power of maintaining its own existence. But in certain cases where it is necessary that it should possess this power, it is attained by preserving such a harmony in the development of the different systems, that they shall all act in unison with one another; and the being does then present a perfect transitory resemblance to those of the class beneath. Thus it would be difficult to demonstrate that the tadpole is not a fish *pro tempore;* no naturalist would hesitate in what class to place it, if only acquainted with its early form; and the same observation will apply to the caterpillar, whose structure is altogether that of the annelides. Taking this view of the case, therefore, metamorphosis does not essentially differ in nature from those changes which every animal undergoes in the progress of its development; but the embryo is adapted for deriving its subsistence from the world around, instead of from its parent, by causing the development of all its structure to go on *pari passu*, so that each organ may harmonize in function with the rest.

Putting aside, however, for the present these extraneous but deeply interesting questions, I proceed to the proper subject of this paper, which is, to apply to *function* one of the laws propounded by Von Bar with regard to structure, namely, that,

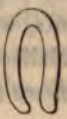
1. A special function arises only out of one more general, and this by a gradual change.

To this law I shall add a second, that,

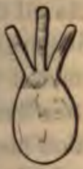
2. In all cases where the different functions are highly specialized, the general structure retains, more or less, the primitive community of function which originally characterized it.

The division of the changes which take place during the existence of the living animal body into the *organic* functions, and those exclusively *animal*, will answer very well for my present purpose, although, as we shall presently observe, it is only in the more specialized forms that we see them distinctly separated. I put aside for the present that series of changes occurring alike in the plant and the animal, which have for their object the continuance of the race, not the maintenance of the individual; these will be a subject for after consideration. The organic functions being common to both kingdoms, it becomes a most interesting topic of inquiry how far the organs which perform them have the same elementary structure in each, how far the changes produced by them are similar, and how far analogy can be traced in their gradual specialization.

As all the changes which are essential to the existence of a living organism may be regarded as consisting in the assimilation of matter from without, and the liberation of excrementitious matter from within, so the two functions of absorption and excretion may be regarded as comprehending the sum of the acts by which these changes are produced. In the lowest plants and animals we find no provision for any more complicated processes. In the Algæ, for example, the whole surface is absorbent; no part more than another can be regarded as peculiarly exercising this function; every cell derives from the fluid in contact with it, or from the surcharged cells in its immediate neighbourhood, the fluid essential to its existence. In like manner we might advert to the structure of the gemmules of the Porifera and Polypifera as furnishing an example of a similar mode of nutrition in the animal kingdom; but as these beings are mere embryos, it is not perhaps fair to adduce them in illustration. We very early find in the animal kingdom a tendency to specialization of the organs of absorption, by the appropriation of a continuation of the external surface for the purpose. In the common hydra, for example, we may regard the animal as entirely composed of a stomach and its appendages; and this stomach may be regarded as simply a reflection of the tegumentary membrane *inwards*, as the experiments of Trembley sufficiently prove, by shewing the mutual convertibility of these two surfaces. We may then express the form of the absorbent portion of the general surface by such a sketch as the following (Fig. 1). Now, although we have here a decided internal stomach, we still have the tissues deriving their nutriment by *immediate* absorption, partly from the fluid within the bag, and partly from that on the exterior, as the experiments before allud-



ed to seem to prove. Here, then, is the lowest degree of specialization of the function of absorption in the animal kingdom; and perhaps we may regard the condition of the absorbent surface in the lichens as somewhat analogous to it, since in these it is generally only *one* surface that absorbs freely, namely, the one least exposed to the sun and air. The first appearance, however, of any extension of the surface for this purpose (such as we find in the radical fibres of some fungi, but more particularly in the mosses), takes place by an *external* prolongation; and we may therefore consider Fig. 2. as illustrating, in contrast with Fig. 1, what may be regarded as the type of the absorbent system in plants. The final cause of this difference in the direction of development will subsequently come to be considered.



Now, it will be remarked, that as soon as a particular part of the surface is modified for absorption, the tissues in general derive their nutriment indirectly through the medium of a circulating system, however imperfect. The organs of circulation are therefore to be regarded, not as essential to our idea of a living being, but as superadded in those cases where the transmission of fluid from one part to another has become necessary. Cuvier endeavoured to shew that the development of the organs of circulation in animals proceeds *pari passu* with that of a distinct respiratory system; I think it is evident, however, that we are to look for the fundamental cause of both in the specialization of the absorbent surface, through which the aliment is introduced which is to undergo subsequent change. We find in the mosses and fungi more or less separation of the nutritive apparatus from the rest of the plant, by a distinct axis of growth or stem; and in this we find the cells elongated in such a manner as to approach the form of vessels. In the Algæ, on the other hand, where there is no necessity for any transmission of fluid, the cells approach more to the normal spherical form; and if one part of the frond be taken out of the water, it will wither, although the rest be actively vegetating. In ascending through the scale of cellular plants, we find the absorbent system becoming more and more specialized, and the vascular communication more complete, until we find in the Phanerogamia the extremity only of the root modified to imbibe fluid, and the nutriment rapidly conveyed by the vessels of the stem to distant parts of the plant, where it undergoes certain processes of elaboration, which render it fit to be applied to the purposes of nutrition.

The compound Polypifera may probably be regarded as presenting us with the first appearance of a distinct circulating system in animals. The motion of water through the pores and canals of the sponges, can scarcely, I think, be regarded in this light, since the proper function of absorption does not commence until the fluid comes in contact with the soft tissue lining these passages; which have been justly compared, by Dr Grant, to the ramified roots of a plant turned inwards. It is in the Echinoderma, however, the bulk and solidity of whose tissues prevent that immediate absorption, either from the stomach or the external surface which prevails in less developed animals, that we first perceive a complete vascular system; and this is employed like that of plants in receiving directly, from the absorbent surface, the fluid aliment, and in conveying it to the distant parts of the organism. We are then to regard the stomach and alimentary canal of animals as organs to which no analogy exists in plants; in the latter, the nutriment is directly received from the surrounding medium, in a fluid form, no solid material being capable of being introduced into their system until first dissolved. Their food, therefore, consists of water, holding various matters in solution; and, I think, is capable of being proved, that water and carbon in some forms constitute all that is essential to the growth of plants. They are, therefore, entirely dependent on the inorganic elements around them; and as these are constantly within their reach, vegetables have no need either of organs of locomotion, or of an internal cavity to store up or prepare their food. Animals, on the other hand, being chiefly dependent upon matter previously organized, which can only be procured under certain circumstances, require peculiar means of obtaining it, and a particular apparatus for preparing it to be introduced into the system. We cannot regard any substance to have been so introduced, until it shall have been absorbed; and the only difference between the skin and the mucous membrane in this respect being, that the latter absorbs with the greatest facility, it is evident, that the aliment taken into the stomach bears no different relation with the organism in general, than when applied to the exterior surface of the body. These views may appear trite and almost self apparent, but physiologists are in the habit of overlooking them.

Pursuing the development of the absorbing system in animals, and the gradual specialization of the function, we find, that, in the higher classes, the process no longer takes place by the general circulating system (as by the mesenteric veins in the Echinoderma), but that a new set of vessels is interposed which is still more peculiarly adapted for the purpose. It would seem that the earliest true lacteal vessels are found in fishes; and they possess many communications with the venous system, both in this class and in the reptiles. Nearly the same may be said of the lymphatic vessels whose office it is to perform interstitial absorption throughout the system. In the Mammalia, however, the absorbent system is still more specialized by the want of all communication with the veins, except through their terminal trunks. By thus tracing the gradual evolution of the special absorbent system from its more general type in the lower classes of animals, we arrive at a knowledge of its real nature.

Let us now study this function in another point of view, by applying to it the second general principle with which we set out. Although the roots of plants are evidently their special organs of absorption, there can be no doubt that the leaves and other succulent parts of the general surface perform this function when the former are absent, or afford a deficient supply of nutriment. In many of the epiphytal parasites, the latter are evidently only absorbing organs; and no one can have observed the effects of atmospheric or artificial moisture on a desiccated plant, without perceiving their importance. That the special function of the leaves is of a totally opposite nature, admits of no doubt; and we have here, therefore, a most interesting example of the principle, that the general surface, even in the most highly elaborated organism, retains more or less its primitive community of function. Nay, in the plant, the leaves possess a peculiar power of adapting themselves to the discharge of this office; for not only do they present a broad expanse of permeable cuticle (I think it probable that absorption of fluid does not take place through the stomata, but by the general surface); but they extend this, when occasion requires, by the formation of numberless lymphatic hairs, which, like the radical hairs of mosses, &c., have a strong attraction for atmospheric moisture. Decandolle has remarked in his Theorie Elementaire, " That when any part of a plant cannot, from peculiar circumstances, discharge the duty appropriated to it, the function is performed, wholly or in part, by some other organ. It is evident, that this is but a result of the general principle I have above laid down; and the reason that plants differ in this respect from animals is, simply, that in the former the specialization of function is in no instance carried so far as in the latter; so that, any part of the general surface can perform, in a considerable degree, the function of all the rest. In the animal kingdom, we perceive that the external surface of most aquatic tribes forms part of the general absorbent system; but that in the inhabitants of the air, its function is partly changed, and it is rather an organ of exhalation. The experiments of Dr Edwards, however, shew the importance of cutaneous absorption both in fishes and reptiles; and the human body, in certain states both of health and disease, is greatly dependent upon it. A curious instance of the extent to which it may take place from atmospheric moisture alone, was related to me a few years ago whilst in the West Indies, by the governor of the island in which I was residing. A jockey, who had been in training for a particular race, being much depressed by thirst, on the morning on which he was to ride, drank a single cup of tea; the stimulus to the cutaneous system was so great, that he increased in weight 6 lbs., of which 5 lbs. must have been from atmospheric moisture. The facility with which absorption takes place through the lungs (which are to be regarded as excreting organs) is another example of the same fact; and I think that the present state of belief derived from experimental inquiry, with respect to the relative functions of the veins and absorbents, might have been anticipated by a knowledge of the principles I have been attempting to demonstrate.

In retracing the ground over which we have passed, we remark, that in the simplest organisms, that both animal and vegetable, a permeable membrane is all the apparatus necessary for absorption; and that vessels only become requisite where the fluid has to be conducted to a distant part, either to serve for the nutrition of the system, or to undergo a change in its own constituents. We have remarked, also, that the digestive apparatus of animals is to be regarded rather as an appendage to the absorbing organs, rendered necessary by the nature of their food and mode of obtaining it, than as forming an essential part of the system.

We shall now endeavour to analyse the excretory system in a similar manner; but here we meet with greater difficulty from the increased complexity of the function. We cannot regard the rejection of the excrementitious portion of the food as a part of the function of excretion as performed by animals, any more than the reception of the food by the mouth is a part of the function of absorption. It would be better, therefore, to limit the term *excretion* to the throwing off matter which has been already assimilated. This process, which is constantly taking place in most of the tissues of plants and animals, bears a strong relation, in point of activity, with the tendency of each structure to spontaneous decomposition. Thus the bones of animals and the heart-wood of plants will exist almost for an indefinite period after the death of the individuals; and in them little change takes place during life. In the softer tissues, on the other hand, whose decomposition is so rapid after vitality is extinct, the processes of interstitial deposition and absorption are vigorously performed throughout the whole existence. Hence it may perhaps be inferred, that the power which living bodies possess of resisting the usual decomposing influences of heat, moisture, oxygen, &c., is due not so much to anything essentially different in the affinities which hold together these elements during life and after death, but to the vital actions by which every particle exhibiting the least tendency to disorganization is immediately removed, and replaced by matter newly assimilated. It is a curious fact that after *animal* life is extinct, a certain degree of *organic* life frequently remains, by which the excretory functions go on for a time; thus carbon is exhaled in considerable quantity from the skin for a certain period after death, perspiration has appeared on the skin, urine has been secreted into the bladder, and it is even said that the hair has grown. It is only when these excretions are finally stopped by the want of circulation, respiration, and other vital functions, that decomposition can properly be said to commence.

With regard to the excretory functions of the lower classes of plants and animals, we have very little certain knowledge. The general surface in them seems to answer all the required purposes; and the first organs of secretion we can detect in animals seem rather appendages to the digestive apparatus than parts of the excretory system. Though some may regard the function of respiration in a distinct light, I see no reason for considering it as anything else than a part of the series of changes by which superfluous matter is discharged from the system. Our present knowledge of the elementary structure of glands reduces the lungs to the same type with the liver or kidneys; both consist of an excretory duct upon the minute ramifications of which bloodvessels are distributed, a part of whose contents find their way through the permeable membrane which forms the tubes or cells; and the branches, although possessing a different form, have evidently the same "fundamental unity" of structure. In regard to their functions also, there would seem no further difference than this, that whilst the excretion by the lungs serves an important purpose, the maintenance of animal heat, that of the liver answers another object by giving assistance in the digestive process. Both have alike for their object the excretion of carbon from the system, and their functions may perhaps be regarded as in some degree vicarious.

The respiratory organs are found specially developed both in plants and animals, as soon as a particular part of the surface is set apart for absorption; and the fluid is brought to them by the circulating system before being applied to the general purposes of nutrition. In plants we always find them formed by expansions of the external surface, beneath which the fluid is exposed to the influence of the air; and this is the type on which the brancheæ of aquatic animals are constructed, whatever may be the modifications of their form and situation. In air-breathing animals, on the other hand, the prolongation of the surfaces takes place internally, so that the air comes to meet the blood, instead of the blood being sent to meet the air. Figs. 1 and 2 therefore will equally well serve as representations of these two principal types of the development of the respiratory system. We find, however, many interesting intermediate forms, such as the pulmonary branchiæ of the Arachnida; and in tracing the development of the air-bladder of fish into the lung of the reptile, and at the same time the progressive disappearance of the gills, we have a beautiful example of the gradual change which (where the links are all within our reach) may everywhere be observed throughout nature. I think that the structure of the respiratory organs affords a beautiful illustration of the argument which might be raised on *a priori* considerations in favour of the doctrine of "fundamental unity of structure."* The function of respiration is a very simple one, and it is essentially the same not only throughout the animal kingdom, but in vegetables also, as I shall presently shew. It might be regarded then, as a necessary result of the law, which everywhere prevails throughout creation, of the attainment of every *end* by the best adapted *means*, that the essential structure of the organs should be the same where the function is the same, but that the disposition of these parts should vary with the circumstances in which that function is to be performed. I need not point out the evident correspondence of this conclusion with existing facts.

The experiments of the late Professor Bennett and Dr Daubeny, on the gaseous changes produced by vegetables, warrant (I think) the conclusion, that the disengagement of carbon, which by union with the oxygen of the atmosphere forms carbonic acid, is constantly going on, and is essential to life equally with the respiration of animals; while the fixation of carbon, which only takes place during the stimulus of sunlight, is rather analogous to the digestion of animals. The latter process in a healthy plant far more than counterbalances the other; and thus the greatest part of the solid matter of the tissues must be obtained, since it does not appear that much carbon is taken up by the roots of plants in general. The fixation of carbon, however, only takes place in the green parts of the surface; and the fungi being entirely destitute of this power, can only vegetate on decaying organised matter, which affords a regular supply of carbon to their radical absorbents, and they give out a large quantity by respiration from the general surface.—Now, although in the highest vegetables, the leaves are the principal organs for effecting the gaseous changes already alluded to, these changes take place more or less by the whole surface, and the access of atmospheric air to the roots is of great importance to the health of the individual. The functions of the spiral vessels of plants are not certainly known, but from the quantity of oxygen they contain, they would appear to partake in the process of respiration.

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* This term I derive from Dr Barry.

In tracing the gradual specialization of the respiratory system in animals, we may perceive that its perfection is marked, not by its apparent extent, but its concentration. Thus the ramified tracheæ of insects, extending throughout the whole system, afford an amount of respiration superior in proportion to that of most vertebrata; but the apparatus is evidently formed on a low type. The same might be said of that of birds, which is extended in a similar manner, and for a similar purpose. The large size of the air-cells indicates the comparatively small extent of surface actually employed for the performance of the function; whilst the minute subdivision of the lungs in the higher mammalia, and their complete enclosure in the thoracic cavity, mark the highest degree of specialization of which this apparatus is capable. In all cases, however, we may observe that the general surface retains more or less of its primitive community of function. In the soft-skinned Batrachians the experiments of Dr Edwards have well demonstrated the importance of cutaneous respiration; and similar experiments on the human body shew that the same process is constantly taking place; and it would appear that where there is much local action, as in inflammations, the quantity of carbon discharged from the skin is very much increased. It would be interesting to investigate if this excretion like that of the liver is increased when the functions of the lungs are impeded by disease.

The exhalation of fluid is another part of the function of excretion which might be separately considered in the same manner; but my limits forbid me to dwell upon it. I must also be very brief with regard to the acknowledged organs of secretion. We find that in plants the secretions are usually formed to be stored up in the system, where they answer some purposes not well understood; a few are exuded from the surface; but it is not a little remarkable that some of the principal excretions of plants take place by the roots, the special organs of an opposite function. I am disposed to believe that this excretion, whose importance to the agriculturist is now acknowledged, necessarily results from the process of exosmose which must exist wherever endosmose is carried on; and as it would seem probable that a part of the descending sap, or of the previously formed secretions, is mixed with the absorbed fluid for the purpose of increasing its density and maintaining the endosmose, it necessarily follows that some of it must be lost in this manner. The greater activity of the vital functions in animals, and the larger quantity of the solid ingesta, require a more special provision for excretion besides that which takes place by the respiratory and exhalant systems. Accordingly we find biliary and urinary organs very low in the scale; but these are still formed upon the same general plan. We see the simplest type of their structure in the mucous crypts of the alimentary canal; and as the excretory ducts are but prolongations of the external surface, so their minute ramifications by which the gland is formed, are to be regarded in the same light. As all the glands, therefore, have the same elementary structure, and differ only in the peculiar adaptation of each to separate a particular constituent of the blood, it is a necessary result of the second law to which allusion has so frequently been made, that either the general surface or either of the special secreting organs should be able to take on, in some degree, the function of any gland whose duty is suspended; and observation and experiment fully bear out this result. The "fundamental unity of the structure" of glands has been made apparent, not only by comparing those of different degrees of development in the same organism, but by the study of their gradual development in the animal scale.

I have now sketched an outline of the doctrine of Unity of Function with regard to the changes essential to the maintenance of individual organisms, both of plants and animals; we may next briefly direct our attention to the reproductive system, and examine how far it is from the first a special apparatus, and whether its functions are completely separated in any case from those of the nutritive organs. In tracing the development of the reproductive organs in the cellular plants, we observe that in the lowest tribes the multiplication of cells may be considered as alike the production of new individuals and the extension of the original organism; each cell is capable of maintaining an independent existence, but each is connected with those around it in forming one general structure. Where special reproductive vesicles are evolved, different from the cells which form the plant, we find that at first no particular part of the general surface is modified for their development; but in the higher algæ, the lichens, and especially the fungi, we can trace the gradual separation of the reproductive from the nutrient system. In the flowering plants we have still two modes of reproduction; the special apparatus of fructification furnishing seeds; and the nutritive system furnishing buds, which may be regarded as extensions of the original stock, or as new individuals. The doctrines of Morphology, however, prove to us that even the fructifying system is but a different form of corresponding parts in that of nutrition; and hence the separation is never complete in vegetables. In the lowest animals, we may remark a similar difficulty in fixing the precise limits of individuality; and this is a necessary result of the gradual specialization which this function undergoes in common with every other. Where distinct gemmules are formed, (which in their homogeneity resemble the spores of cellular plants,) they are at first produced from any part of the external surface, as in the hydra; but in ascending the scale we find a particular apparatus adapted to the evolution of the embryo, such as the curious ovaries of the echinoderma. As we advance, the structure of the ovum becomes more and more complex, and the analogy which its parts bear to those of the seeds of plants is sufficiently obvious. I may, however, point out the correspondence both in structure and function, between the cotyledons* of plants, especially such as are membranous) and the temporary branchiæ, which, as is now well known, may be traced in the embryos of all the higher animals.

That the embryo of flowering plants takes its origin in a simple vesicle, analogous to that which forms the entire germ of the cryptogamia, is now well understood; indeed, I think it would be easy to apply in detail to vegetables the doctrines of "fundamental unity of structure," which Dr Barry has shewn from the researches of German embryologists to exist throughout the animal kingdom, and which he has spoken of as applicable to organised beings in general.† One speculation I may hazard at the present time, leaving it to abler botanists to decide upon its merits. The embryo of flowering plants continues to be developed during the ripening of the seed, so that at the period of maturity the cotyledons are fully formed, and the plumula and radicle are ready to elongate themselves into an ascending and descending axis. The spore of a cellular plant, on the other hand, being a simple cell, first produces others similar to itself, and these gradually form a leaf-like expansion, such as Mirbel has beautifully shewn in the marchantia, and such as exists at a certain period in the germination of ferns also.‡ This leafy expansion it is from which the stem, roots, and gyrate fronds of the latter class originate; and when these are fully formed, it decays away. Although the mode of the development of the stem seems in Mr Dickie's view to prevent us from regarding this leafy expansion as a cotyledon, I scarcely see how we can regard it in any other light; since, physiologically speaking, it differs only in this, that the embryo of the fern forms it whilst maintaining an independent existence, and the embryo of the flowering plant whilst supplied with nutriment from the parent. The analogy, then, between the spore and the seed, is something like that of the tadpole and the frog, both of the former being less developed states of the latter, but modified to maintain an independent existence. Taking this view of the case, the fronds of such plants as the marchantia, which are permanent, and never develope a distinct axis of growth, must be regarded as cotyledons, and are obviously analogous to the perennial branchiæ of the lower classes of animals. It is interesting to remark that the simple cell, which is the type of the lowest plant as well as of the lowest animal, is also the type of the earliest embryonic condition in both kingdoms; and there is no more perceptible difference between the germ of a plant and an animal, than there is between those of the different classes of either kingdom.

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* Where the cotyledons are *fleshy*, that is, contain a store of albumen within themselves, they evidently supply also the purpose of the yolk-bag in the ova of animals.

† I think it due to myself, however, to state, that, as far as regards the vegetable kingdom, these views were previously entertained and expressed by me, although Dr B. was, I doubt not, unaware of the fact.

‡ Magazine of Zoology and Botany, vol. i.

In tracing the gradual development of the functions peculiar to animals, namely, sensation and voluntary motion, we may, I think, even here find that the special type is evolved from one more general. If the views which I have elsewhere stated be correct, it follows that the irritability of certain tissues in plants is analogous to that of the muscular fibre of animals; and that the actions immediately connected with the maintenance of the organic functions of the latter, are the direct result of external stimuli on their organism. The possession of a nervous system must, I think, certainly be regarded as the distinguishing characteristic of animals, although our means of investigation will not always enable us to detect it; but the functions of this system in the lowest classes of animals, would appear to be almost entirely confined to the conduction of impressions from one part of the organism to another. As we rise in the scale, we observe that the instinctive actions which are the necessary respondence of the organism to external stimuli, are gradually overpowered by the influence of the will; and although, as has been recently observed,* we may regard the nervous system as living and growing and carrying on its actions within the body of an animal as a parasitic plant does in a vegetable, and as not communicating any influence which is immediately essential to its organic functions, yet we must perceive (to use the words of the same author) "that the objects of the existence of animals require that the mental actions of the nervous system should exert a powerful controlling influence over all the textures and organs composing an animal."

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* British and Foreign Medical Review, vol. iii. p. 10.

The organs of sensation, when examined in the ascending scale of animals, will, I think, afford us illustrations of the same general principles. We may perceive that the special functions of sight, hearing, and smell, are rather elaborated out of the general sense of touch than superadded to it; and there would not therefore appear, *a priori*, any physiological impossibility in the fifth pair supplying a certain power of sight when the optic nerve is absent, as in the mole; and, if the phenomena of the transference of sensation should ever be indisputably established, their explanation on the same principles will be easy.

Without entering into any detail with regard to the structure of the various organs of locomotion in animals, it is easy to observe the intimate connection which exists in the lower classes of this kingdom between the exercise of this function, and those movements which are essential to the maintenance of the organic life. Thus the cilia, which in so many of the aquatic tribes are almost the sole instruments of progression, serve also to bring supplies of food to the mouth, and of water to the respiratory organs. In the higher classes we see each of these functions performed by a special apparatus, but still a connection may be traced; and I know not a more striking illustration of it than the structure of the locomotive system of birds. In this class, as in insects, a high amount of respiration has to be combined with general buoyancy of the body; and this object is attained by the general diffusion of the respiratory organs. But in insects, the principal organs of progression are merely an extension of the respiratory system; whilst in birds, a special locomotive apparatus is evolved, which still, however, retains a certain connection with the function of respiration.

On certain Modified Forms assumed by the Inductive Process in different Sciences; being an attempt to elucidate and extend some Doctrines of the Novum Organum. By ROBERT MORTIMER GLOVER, Esq. (Communicated by the Author.)

OF late, great importance has been rightly attached to the cultivation of those doctrines in primary philosophy, which regulate the formation of our practical rules of scientific inquiry. Indeed, as the doctrines alluded to are axioms including nearly all science in their relations, although in themselves of an abstractedly simple nature, it is very needful for the notions entertained with regard to them to be explicit. The proper functions of experiment and calculus, with other topics of no secondary consequence, can be thoroughly understood, only when investigated in their connexions with the theory of the Inductive Logic. And the Novum Organum itself exists as an everlasting memorial of the utility which may at any time result from the attention of scientific men being directed to the study of the laws that govern the mind in acquiring its knowledge, and the bearing of these upon actual inquiry:—for in the work is displayed the mode in which its immortal author was enabled to frame a code for future investigators, and perhaps to alter the bent of the energies of his time, from having conceived more enlarged views of the province appertaining to induction, than those possessed by his predecessors. In the present day, the light derived from numerous successful efforts to explore nature, has been reflected upon the study of methods and systems; and many illustrious disciples of the Baconian school in this country and abroad, have thus been able to refine greatly the precepts of their master. It seems to be generally supposed, that the labours of Stewart, Playfair, Laplace, the present Herschel, and others nearly equally eminent, who have treated of the applications of the Baconian philosophy, have almost exhausted the subject, and that if little can receive further elucidation, still less remains to be explored. But we believe, if the opinions of writers on induction be rigidly examined, a greater want of unanimity among them, even on very essential points, will be detected, than could be credited before hand. In particular, writers on logic are often by no means in accord with those who have described induction not exactly as a form of mental procedure, but by the help of certain signs in nature correctly supposed to correspond with successive steps in the mental operation. Besides, much ill defined language is currently used in speaking of the inductive process, its character and relations,—thus, it is often said, that physical laws have the power of enabling events to be anticipated by means of *others* which *have been* observed; and this is asserted sometimes when perhaps no very clear ideas are entertained of the *character* of this curious property attributed to physical laws. Further, the category of inductive sciences seems not very accurately defined. Metaphysicians have debated among themselves whether the precepts of the Baconian philosophy are properly applicable to the science of mind,—and whether the investigation of mental phenomena can be considered to involve the practice of a method of procedure at all analogous to the experimental inquiries of physics. And although in the northern part of our island, these questions have been answered in the affirmative,—a response has not been so generally given by the English and Continental metaphysicians. In like manner, writers on the philosophy of medicine differ greatly as to the extent of application admitted in their science to precepts which have been found invariably fertile in results, as applied in pure physics.

Our object in this essay is to make an effort to reconcile some of the above stated discrepancies, and to clear up (if possible, other portions of the theory of the inductive logic which appear to us in need of elucidation; and these intentions we purpose to effect, by detecting and defining certain forms which induction seems to assume in different sciences, when that process is regarded not in the mind, but through its corresponding signs in nature,—and which forms do not appear to be distinguished as yet in a clear manner by writers; while, at the same time, we endeavour to show how those varieties came to be, as it were, developed out of the fundamental process which is performed in mind,—which does never vary in essential character, whatever phases it may assume, when the indices correspondent to it are regarded in nature. As our space is necessarily somewhat limited, we shall only premise further, that our purposed divisions will regard methods of inductive procedure, and not individual instances, as in the classifications of Bacon, and that if the circumstances in the very constitution of the different sciences, which compel the inquirer to take diverse routes in arriving at their truths, have already been described, the subject, so far as we are aware, has not been treated as a whole in the particular way proposed.

It is perhaps scarcely proper to remind the reader, that all our knowledge is rendered available to the reasoning faculty by means of what is termed generalization:—for, as all processes of pure reasoning may be resolved into syllogisms, which can proceed only from generals to particulars,—until the mind has arrived at general notions, it cannot of course be capable of reasoning either on the subject-matter of the knowledge afforded by scientific inquiry, or on that of the information acquired in the ordinary relations of life. To that intellective faculty which has the power of forming general notions is given the name of abstraction; while its mode of procedure is termed induction or the inductive process. Abstraction is not regarded by metaphysicians of the present day as a simple faculty of the mind; but its real nature is of little concern here;—let it be understood, however, that induction is its mode of procedure, and generalization its result. And, first, let us attend to the result, in order that a clear conception may be had of what is required in a method of procedure, the great object of which is that this result may be attained.

In a logical point of view, a science may be regarded as a collection of general terms, each of which in all sciences, except those generally considered abstract, expresses common circumstances possessed by a certain number of particulars, from the examination of which the genus has been formed. In the abstract sciences, as for example in geometry, and that department of mental science, which, by the disciples of Reid, is called the doctrine of first truths, the highest and most inclusive principles are ideas of relation which subsist solely in the mind itself, and are found therein; so far, therefore, in those sciences there is no occasion for an inductive process, since the most general facts are also the simplest elements of belief, and as in geometry the only further foundation requisite for the whole series of truths composing the science, is, that some purely intellectual forms be described by references to those elementary principles, the science is altogether independent of induction. But, in the study of nature, both external, and within ourselves, all science (except the above mentioned portion of mental science, and perhaps a corresponding part of the doctrine of ethics) requires an analysis of a mass of phenomena, which at first sight appear exceedingly heterogeneous and complicated, in order that they may be resolved into simpler combinations, which, however, are expressed in terms more inclusive the farther the analysis is pushed. In other words, after the whole of the universe has been resolved into separate and distinct parts, these are again combined by the mind, and arranged into mental *loci*, according to laws furnished by itself. The grand object of this system of arrangement is not that the purpose of distinctness may be answered, nor that knowledge may thus be properly treasured up, but it is that this knowledge may be reasoned upon, in order, in fact, that the intellective process, which is carried on in syllogisms, may take the place of that which constitutes induction; and that those wonderful effects may be produced which flow from comparing and combining the results of human inquiry.* Such being the case, it may be conceived, that in framing those genera, the mind is not compelled to take notice only of such properties as are believed most essential to the constitution of the individuals to be grouped together; on the contrary, the abstraction may be of whatever properties are chosen, according to the notions entertained of their fitness for an end in view. All that is absolutely necessary to be attended to in the formation of a law, being that the properties fixed on for its types have an actual existence in all the particular instances composing the included genus. Thus, the Linnæan arrangement of plants is as just a system, so far as the mind is concerned, as that of Jussieu. It is true, the one system takes cognizance of a greater number of characters in composing its genera than are considered in the classes and orders of the other, and also of such characters as are believed most essential to the very nature of the individual plants. This system is therefore physically the more perfect of the two, but it is not therefore more logical than the other. In that respect both are alike,—both sufficiently accurate in logical structure, but framed for different ends.

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* It is properly remarked by Whately, that those who propose in teaching logic, to substitute the Organon of Bacon for that of Aristotle, show a total want of comprehension of the intentions of either. This may be placed in a very strong light, when we reflect, that without the exercise of syllogistic reasoning, Watt would have been unable to apply the inductively raised laws of Black to the improvement of the steam-engine,—a fact which we could *easily prove, were* there space or occasion at the present time.

The preceding observations may in some degree illustrate a great maxim of the Kantians, which makes the fundamental principles of all science repose in the intellect itself;—asserting the human understanding rather to dictate the laws which regulate its acquisition of knowledge, than receive them from the external world.* Indeed all general notions are the workmanship of the mind, and often cannot be ascertained to correspond exactly with existences and actions of nature. And this, even, on the understanding that such notions are derived in all cases by an exercise of the intellective faculty from real impressions. For example, the intellectual forms which are the objects of geometrical reasoning, and which, being ideas of relation, have somewhat of the character of general notions, are not to be found pure in nature. And something similar, or at least analogous, holds of physical laws also; for these are either general terms signifying the agreement of a number of particular facts or phenomena in some common properties, or else abstractions of some actions of Nature from others with which they must in many cases be viewed in their real state, somewhat combined.

The characteristic, or what may be termed the logical characters of all physical laws are similar. For a definition of a physical law, in logic, it is enough to term it a statement implying a connexion observed between some properties and others, in any definite class of instances. It is quite essential, that the class of facts to which a law is applicable should be defined, but all the particular instances included in that class need not be known.[*] By the term property is meant a structure, a quality, or a function. Our observations with regard to laws apply to such general expressions as include *all their particulars* with logical certainty; and if in any place another meaning be attached to the term, it shall be stated explicitly.

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* Philosophie de Kant, par Villiers, p. 301, 8vo. Metz, 1801. The same truth is elaborately illustrated by Dr Brown in his 5th lecture.

On examination, the above definition will probably be found to include every thing absolutely requisite to constitute a physical law; and to be so general, that scarcely a law will be found without some physical or metaphysical properties superadded to those in the definition. But when illustrations are sought, they will doubtless be found in accordance with our statement. Thus, to take two examples differing in some respects from each other; the series of laws composing the Newtonian doctrine of gravitation, and the generic terms of the method of natural families of plants, present laws expressing generally, in the one case, the fact, that matter, in separate masses, and within certain observed limits, has been found endued with the property of gravitation,[†]—and in the other, that in certain groups of plants, the individuals resembling one another in form, do also agree in respect of medicinal and culinary virtues.

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[*] This distinction is believed to be of great importance; perhaps the following illustration may explain more fully its nature. The first law in the theory of gravitation was proved nearly as we are about to mention. Galileo found experimentally a few balls of different materials to obey the law of gravity within very short distances of the earth. Newton proved by calculation, that the same law extends to the moon. And by trials of very dissimilar substances on the surface of our planet, it seemed to be made out that the property of gravitation could belong to them only because it belongs to all matter in their circumstances; those bodies being so unlike, as that they could scarcely owe the property to anything, except a common material nature. Thus, when the law of gravity is stated as holding true of matter within certain limits, it includes in its expression (with logical propriety) innumerable individual instances, many of which probably may differ somewhat from the instances originally experimented on. But if the grounds of the original conclusion were correct, this latitude of expression cannot be objected to.

[†] Dr Brown first shewed, in his usual forcible manner, that the law of gravity could not be extended with logical propriety or physical certainty, beyond observed limits. See sect. 8, p. 177 of the 1st vol. of his Lectures, 8vo ed., 1820.

Subordinate to the great logical characters of physical laws, there are other characters of a physical, or a metaphysical kind, which, not being all common to such expressions, but some of them peculiar to particular laws, serve to distinguish those. The chief of these characters depend upon the relations preserved between properties connected in laws, in time, and in space. Characters deduced from such relations, may be termed metaphysical: their existence was clearly pointed out by Dr Brown. The physical characters belonging to laws are very various; and in this inquiry may be considered accidental or contingent.

A law, stating that the properties to which it applies preserve a relation one to another, so that the presence of none is antecedent to the appearance of the others, is an expression like the description of a natural family, or the theory of the circulation reduced to its utmost simplicity. For as, in this latter case, it is only stated that the performance of the circulation of the blood is a function essentially connected with a peculiar structure,—the prior presence, whether of structure or function, is left undetermined. But where one property precedes another in the order of time—one being a uniform antecedent, and the other a uniform consequent, *i. e.*, when this order is found to occur regularly in certain contingent physical circumstances, —the law is then one of cause and effect. Since the phenomena of gravitation are now found to be consequent upon the relative position of gravitating masses, because the attractive influence requires time for its transmission, the law of gravity is one of cause and effect. Matter, in separate masses, and these at distances not extremely minute, being the uniform antecedent;—the phenomena of gravitation the uniform consequent; and the occurrence of the law within certain definite distances (added to what is said just above), the contingent circumstances.

But besides the statement of an existing relation between properties, a physical law often carries with its terms the presumption of an existency in nature, more remote and subtile than the observed properties, and which causes those to preserve their known relations. In other words, the law excites in the mind the idea of something which not being really discoverable in nature, is nevertheless believed to exist. Thus, a law, expressing the relation of properties, as cause and effect, gives rise to an idea of power, or of a mutual adaptation between antecedent and sequent,—the cause of observed phenomena. This latent adaptation is meant when an attractive force, enabling the masses of matter to act on each other reciprocally, is spoken of. The law of gravity does not state the existence of any such adaptation, but merely tells a bare fact. As, however, the human intellect feels inclined to give a reason for everything it discerns, and the occurrence of gravitation being an inexplicable fact as beheld, by the invention of a hypothesis which does assign a reason for the fact, a mode is thus contrived to harmonize the actions of nature with the constitution of the mind. In like manner, when in the system of natural families, a very curious connection has been established between outward conformation and internal properties, it is perhaps impossible to refrain from believing that this connexion has a cause in some more intimate organisation of the plants in which it may exist, or in the nature of the principle of life itself, which may thus hold together by an appropriate bond of union two sets of properties, which, in their known natures, furnish no reason for the actual relations they may maintain. The belief in the existence of ultimate principles is derived from experience; it arises from the discovery of the causes of events in preceding cases; and when any inexplicable connection consisting in nature is detected, there is an instant tendency in the mind to suppose a reason for it.*

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* It will be seen from what is said, that we are of those who assign to vital principles a place in physiological inquiry. There is not space here to enter into a discussion of that question, so involved in controversy; but we will ask those who deny that such ultimate principles can have *any* place in philosophical inquiry, how they can account for such a fact as that given in a recent paper on development, by Dr Barry, viz. that all animal germs are fundamentally the same,—or that, from structures essentially the same, exposed in the Universe to circumstances utterly unable from diversity to produce such diverse creatures (as can be found experimentally), all the varied rms of animalized beings are developed? There can only be one answer: The differences amongst germs which give rise to such dissimilar beings, ist in their principles of life! or are differences in innate susceptibility.

Our opinion, as given above, is in a great measure opposed to the well known doctrine of cause and effect, promulgated with so much eloquence by Dr Brown: but it is now by no means an act of daring to venture the avowal of more speculative notions, with regard to cause and effect, than those published by that justly celebrated metaphysician. For at present his theory is generally dissented from; and we believe that it is not in accordance with the genuine spirit of the Philosophy of Bacon, nay, that brought to bear upon actual inquiry it would be found to have an effect extremely prejudicial.

According to Dr Brown, all that can be conceived, or rather all that should be conceived by the mind of cause and effect, is the invariable antecedence of one property with the consequence of another, under certain contingent circumstances. Besides this invariable relationship, no idea of power or force should be conceived. And, by way of illustration, he analyzes, with his wonted elaboration, the law of gravity, in which he says, all that can be rationally or philosophically conceived of the phenomena of gravitation is stated, viz. the simple fact. Or, to the statement of the simple fact, according to his doctrine of causation, a sound philosophy ought not to attach any hypothesis of the existence of a principle connecting together the properties which are the subjects of that law: the supposition that the phenomena of gravitation are owing to an attractive force exerted between masses of matter being unwarranted by the known facts. Such must be his meaning; and accordingly he censures the query in which Newton couched *his* belief in the existence of an ultimate principle,—the cause of gravity.

Were one, unacquainted with the Newtonian Philosophy, and likewise with any theories of causation, to behold two masses of matter gravitating to one another, he would naturally ascribe the fact to the existence of an attractive force, or the exercise of a secret sympathy; and, if one of the bodies were drawn more towards the other than this one unto it, he would suppose the power residing in the one to preponderate over that power residing in the other; for the feeling in ourselves of what is required to produce analogous effects by muscular exertion, is alone sufficient to produce both convictions. The hypothesis of the existence of an attractive force is admitted by Condillac to be a forced conviction of the mind, and therefore allowable in a philosophical sense also.* Dr Brown himself nearly admits at one time all that the most speculative transcendalist could desire to have from him in favour of the legitimacy of researches into the nature of ultimate principles; for while lamenting the defection of Newton from sound philosophical views, with regard to the proper objects of physical inquiry indicated by the query as to the cause of gravity, this circumstance is attributed by him to the influence of a "human infirmity," from which the greatest minds are not exempt.† The advocates for transcendental or speculative inquiry, when inductive investigation is apparently pushed as far as possible, merely maintain, that, from the very constitution of the human mind, it is not possible for us to refrain from attempts to acquire some notion of existencies in the being of which we are compelled to believe, although they themselves be not before the senses. Both Dr Brown and Mr Stewart ‡ regard a conviction of the existence of something to be explored, as the legitimate and necessary precursor of scientific inquiry: hypothesis is the stimulus to investigation which in the human race as a whole, and in individuals, has ever been urged on by the presumption of success thus afforded. And if the existence of some principle beyond such a law as that of gravity were not supposed, there could be no inducement to inquire after such an entity. Now, the laws of chemical affinity are exactly correspondent with that of gravity; and Davy succeeded in determining the dependence of chemical affinity upon the electric states of bodies. Nay, of late, Mossotti has by abstract reasoning generalized all the phenomena of attraction and repulsion, whether of a mechanical or chemical character, into actions of a common hypothetical principle, which must coincide with the cause of gravity.

Physical inquiry may be regarded not improperly as a constant struggle on the part of the mind, to acquire such a perfect knowledge of the phenomena and scheme of external nature as it has of those ideas of relation generated within itself, which form the basis of geometrical science. Over such ideas its control is complete; it developes them into propositions according to the laws of its own constitution. Now, the formation of general notions is one step towards the reduction of physical science to so complete an intellectuality. But the real or essential principles of connexion between properties have not yet been discovered. Could they be known, physics would become a demonstrative science. But the mind endeavours to supply their place, by supposing the existence of such principles as the cause of gravity. And be it noticed, that those ideas of power or force are like the fundamental principles of geometry, ideas of relation which, according to Locke, have their birth in internal sensation or reflection, *i. e.* in the intellect itself.

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* Traité des Systêms, vol. i. pp. 240–2.

† See Brown's Lectures, vol. i. sect. 8, p. 167.

‡ See Stewart's Lectures, vol. ii. c. 4, p. 403.

It is generally believed that Bacon banished the study of causes from his philosophy. So far is this from being the real state of the case, that, on the contrary, he created a branch of philosophy, the express object of which he makes to be to inquire into their nature. In fact, while the ancients vainly endeavoured to arrive at a knowledge of ultimate principles by speculation, and from such principles assigned hypothetically to deduce effects, and thus to demonstrate all the real phenomena of nature out of their own unassisted reasoning, as was afterwards attempted by Descartes, Bacon proposed first to investigate the real existing and observable connexions among properties, and not to speculate until this investigation had been carried on as far as possible. He separated the study of causes from the study of observable actions, and assigned the former to metaphysics, and the latter to physics.* And Newton, deeply imbued with the spirit of Bacon's philosophy, followed its precepts to the letter, when, having arrived at the law of gravity inductively, he began to speculate as to the nature of the cause of gravity; at the same time defining most distinctly in his 28th query, the true aim of philosophy to be the determination of such lofty inquiries as that which regards the cause of gravity.

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* Since the above made statements may be supposed to involve controvertible matter, we shall support them as fully as our limits permit. In the first place, then, it is sometimes not very easy to get at Lord Bacon's meaning, even when that ought to be exceedingly clear. This has been remarked by Mr Stewart, who says, " In one passage he approves of the opinion of Plato, that the investigation of FORMS is the proper object of science, adding, however, that this is not true of the FORMS which Plato had in view, but of a different set, more suited to the grasp of our faculties." This is nearly the language of the Novum Organon, (Part 1, sect. 2, aph. 51). And elsewhere Bacon declares, that he understands by the word Form the law through which the actions of individual bodies are performed (Nov. Org. P. 2, s. 1, aph. 2).

Physical inquiry consists in seeking after connexions between properties existing in nature, or in endeavouring to discover where such connexions cannot exist. Hence there are negative and positive laws. But all definitions of laws, by an appropriate and slight change in expression can be made to apply to negative as well as to positive cases. Perhaps sufficient has now been said with regard to the results of inductive inquiry, to enable all the varieties of inductive procedure to be understood.

We turn, then, to view the objects of contemplation proposed on introducing our subject. And since a law framed by induction should include the class of facts to which it is applicable, in a perfect manner, every legitimate species of induction must be capable of affording complete proof. But it is evident, that, in such a case as the law of gravity, every individual instance included in the expression cannot be examined, or the labour of proof would be illimitable: how, then, is the requisite degree of evidence in such a case obtained?

Aristotle believed it necessary, for every particular instance subject to a law to be examined, before the expression could be

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But elsewhere he evidently means by FORMS the most remote principles that we can conceive. Thus, he tells us, that the " FORM of any nature is such as, that where it is, the given nature must infallibly be;" (Nov. Org. P. 1, s. 2, aph. 4). And although, in the very next sentence in which the passage we have just rendered occurs, he seems to allude to something still more essential than a FORM, yet, as in the inquiry after the FORM of heat, he concludes heat to be an " expansive bridled motion, struggling in the small particles of bodies;" we think that his forms do also correspond with such principles as the cause of gravity, or the cause of light, but that he has another inferior set of FORMS, such as physical laws. And as he divides his philosophy into the study of FORMS or metaphysics, and the study of effects or actions up to the FORM which he calls physics, proposing, by means of the knowledge acquired in physics, to produce all sorts of mechanical actions, (Nov. Org. P. 2, s. 1, aph. 9), his meaning is thought to accord with the interpretation above given.

logically certain: "Nam inductio fit ex omnibus singularibus."* But the examples of induction with which he was acquainted, were cases in which it is requisite to examine every instance belonging to a law before the due amount of evidence can be collected. As the subject is placed in an exceedingly favourable light by some sentences of Gassendi, we shall take the liberty of quoting those:—

"Etenim ipsa quoque inductio syllogismus reipsa est; et quadamtenus quidem mediæ inter enthymena et gradationem conditionis,—* * * hic cum dicitur, v. c. omne animal gressile vivit, omne item volatile vivit, omne etiam natatile, omne reptile, omne zoophytum; igitur omne animal vivit; assumptiones hinc plures sunt, justa generaliores species gradus animalis collectæ, et quasi in unam coadunatæ, quam ista propositio intelligatur præcedere, omne animal aut gressile, aut volatile, aut natatile, aut reptile, aut zoophytum est.

"Scilicet, nisi hujusmodi propositio supponeretur, suppressare licet, subintelligeretur tamen, consequentioris vis nulla foret; cum si præter enumerata existeret aliud quodpiam animal, conclusio evaderet falsa.

"Unde et licet intelligi, debere inductionem, ut legitima sit, continere omnium specierum, partiumve enumerationem; ne si una quæpiam deficiat, ea exceptionem faciat, probationemque labefactet. Quanquam, quia ut superius semel, iterumque monuimus, difficile plerumque, aut impossibile etiam est enumerationem omnium fieri, dici, aliquibus enumeratio, solet, quod Lucretius, et Horatius, cætera de genera hoc; supponendo videlicet, præter membra enumerata occurrere nullum, quod secus se habeat."†

He goes on to say, that there may be an induction, concluding in the negative, as well as an induction concluding in the affirmative. Except the error of supposing induction to be a species of syllogism,‡ the above passages give a sufficient notion of the opinions entertained at present with regard to the mode of procedure proper in induction, in order that the evidence may warrant the conclusion. Dr Whately, in terms synonymous with those of Gassendi, asserts that an inductive inference, drawn from a part of a class with regard to the whole, can only be supposed legitimate through a species of logical fiction, in assuming that one or two of a small number of instances do adequately represent the class to which they belong. Now, we maintain, that, in the greater number of inductions performed in physical inquiry, such a supposition involves no fiction, but rather a positive fact; a class of facts, the individuals of which are in external aspect somewhat dissimilar, being often sufficiently represented for all the purposes required in the induction by a very few instances.

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* De Inductione (Analyt. Prior. lib. 2, c. 23.)

† Institutionum Logic. P. 3, Canon 11.

‡ An error not confined to this writer, as is shewn in a very powerful article in the Edinburgh Review, vol. lvii.; but common to him with most authors on logic; although not participated in by Aristotle, according to the reviewer. We find Aristotle say, that "quodam modo opponitur inductio syllogismo." And in his chapter on Induction, he seems to assign distinctly the province of demonstrative reasoning to syllogism, and the discovery of physical truth to induction. When he says that syllogism is "natura prior et notior," he probably alludes to reasoning from an obscure and unanalyzed whole to its parts.

The more essential properties of bodies are the objects of scientific investigation; and it is probably only where the induction has regard to such properties, that one fact can be taken as a specimen of others analogous with it. Now, there are instances created in nature with the properties common to their class, so highly developed in them, as that the relations of those can be more readily discerned. And often by the aid of experiment, those properties can be so tested as to enable it to be known, that, in the instance experimented on, where one property is placed in a certain situation, another will attend it in a certain order. And the mind having such a knowledge of a class as to be able to divest its individuals of their accidental properties, and to discover in them one essential arrangement, defines the whole class to possess that arrangement,—which being found, in a certain number of well-marked cases, dependent upon the relationship of some properties, is supposed, upon the ground of an intuitive conviction of like effects being owing to like causes, to be dependent upon the same properties in all the other cases. If the primary definition did not include the whole class, neither should the last inference affect the whole class; in that case, the extension of the conclusions beyond the individuals known to possess a defined arrangement, would be a mere presumption. But let it be clearly understood that there is a power in induction to determine the nature of individual

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* See Whately's Logic, Art. Induction throughout; also some strictures of Mr Stewart in the 2d vol. of his Lectures, p. 345, upon a passage from Dr Wallis of Oxford.

instances which may not have been wholly examined, by means of an investigation of other instances apparently only analogous. Thus, let us suppose, that a definition of the term animal were formed, stating that an animal is a being possessing sensation and voluntary motion; and that it could be found, by a comparison of some of the different grades of animalized being, that just in the ratio of the development of nervous matter, was the state of those functions, in an increasing or a decreasing ratio; and that where this nervous matter exists (as in the genus Echinus and the sub-kingdom Acrita, generally banished by naturalists of the present day from the animal kingdom) not in the form of filaments connected with a common centre, the functions are wanting; would we not be entitled to conclude, that, in all animals, the development of a nervous matter in the filamentous and radiated form, bears an exact ratio to the aforementioned functions? If we do not admit that a few instances can be taken as specimens of a class, containing individuals apparently dissimilar, then, indeed, the mode in which the mind arrives at laws in physical investigation is often incompatible with logical propriety,—a proposition truly monstrous!

But it will doubtless be asked, in what way a conviction can be got of the essential nature of connexions investigated in Nature, when, in the properties themselves, no reason for this essentiality can be detected. Our answer is the admitted aphorism, that we are compelled, by the very constitution of our minds, to take the constancy and invariableness of relations observed among properties, as warrants of the essentiality of the order of relationship. Could the true principles of essentiality,—the FORMS of Bacon and of Aristotle—be discovered, we should then have reasons from which it might be possible to know the extent to which certain relations, observed in a single instance, could reach throughout Nature; and our knowledge of the external world would be a perfect knowledge, so far as it should go. But so long as those principles remain undiscovered, the logic of physics must owe its coincidence with fact to an admission, on empirical grounds, of the existence of an essential series of phenomena, where there is but the evidence of their observed order being constant. We have said, however, that the mind endeavours to supply this deficiency in its evidence with respect to the actions of Nature, by feigning the existence of such principles of essentiality, the want of which it must perceive. And thus the necessary defect in inductive evidence, which many writers on logic have misplaced, arises from no logical fiction or physical impropriety in regarding one individual of a class as a specimen of its brethren, but from an imperfection in the media of communication between the understanding and external nature.

Instances which possess an organization so highly organized as to admit of them being taken for specimens of the class to which they belong, are termed by Bacon prerogative. And it is the chief merit of his philosophy, that he perceived their place and their power.* He has not, however, given any clear general definition of them, but abundant examples to shew their utility; and also a classification of individual instances, in which all the varieties of prerogative facts are minutely described. Here we shall consider them in general, since our object leads us to view them, not as they differ among themselves, but according to their place in the general theory of induction.

A good illustration of a prerogative fact is afforded by the famous experiment of the soap bubble, by which Newton discovered, in a single instance, the proximate structure upon which the various colours of all bodies are dependent. This instance is composed of several parts. In the first place, an appearance of coloured rings was observed upon the surface of the soap bubble, and their order of appearance in some degree estimated and compared with the thickness of the bubble at different parts; in the second place, the instance was varied by using a layer of water placed between the object glass of a telescope and a flat surface; thus the thickness of the layer of water could be measured, where the different rings appeared, and also the order of their array became more regular; in the third place, it was found that the condition of the ambient body did not affect the order of appearance, although it did the strength and variety of the colours; in the next place, it was found that different transparent bodies would not, under the same thicknesses, exhibit the same colours; lastly, it was discovered that the transparent body viewed obliquely, did not exhibit the same colour at the same place, as when viewed directly, and that bodies undergo changes in colour by alterations in their mechanical condition (as well as can be observed) according to the law, which at this part of the inquiry might be supposed to exist. This famous experiment enabled Newton to frame a law, which states that the causes of the different colours of *bodies* exist in the sizes of their component particles.

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* Nov. Org. P. 2, s. 2, Aph. 22.

When one instance does not afford a sufficient display of properties to allow of an extensive inference being drawn, other instances must be got together, which in a mass have such varied characters as to make up a strong case. Thus, in Dr Wells's theory of dew the great doctrine of which is, that in all bodies on the earth's surface, the dew-attracting power bears a uniform ratio to the power of radiating heat; the author proves his main fact, by taking platinum, gold, silver, lead, charcoal, grass, and gravel, with such like instances; cases which, put together, may be supposed to afford a fair specimen of the relations preserved by the properties in question throughout all bodies whatever, there being among these instances every conceivable variety in radiating power from the zero of burnished silver to the maximum of porous charcoal; and he finds, that, in the cases experimented on, the relation between the properties is regular. He therefore draws an inference for all bodies in the circumstances of those which he has tried.

When induction, therefore, takes cognizance of the more essential properties of bodies, and investigates those by means of instruments, an inference including all facts in certain contingent circumstances can be drawn from a few analogous facts in such a condition; and the mind, in framing this it may be universal generalization, does not necessarily make use of any fiction, logical or physical, but proceeds upon what may often with reason be believed to be a sure fact.

But where the properties are not of this essential and intimate kind, and do not admit of being investigated experimentally, so as to allow of their relations being determined, a universal conclusion cannot be drawn without the use, by the mind, of that logical fiction of which so much is said by Dr Whately and others. Thus, to give an example which may perhaps illustrate an oversight of writers on induction, we quote the following from Dr Brown:—"If, by the term general law," he says, "be meant the agreement in some common circumstances of a number of events observed, there can be no question but the view is a just one, and that what we have already found in a number of events, may be applicable to that number of events; in the same manner, as, after having combined in the term animal the circumstances in which a dog, a horse, and a sheep agree, we cannot err in applying the term animal to a dog, a horse, or a sheep. But the only particulars to which we can, in this case, with perfect confidence apply the term animal, are the very particulars before observed by us."* Now, here we perfectly acquiesce with the argument of Dr Brown: but it is sufficiently obvious, from the examples already given, that his observations do not apply to all cases of inductive inference. Logicians generally give examples of inductive generalization, which do not shew the occasional and frequent power possessed by facts of exhibiting the properties of their class in a distinct point of view, as in the above instance of Dr Brown. But let the example produced by him be contrasted with the one given by us of the mode in which a correct notion of the connexions and relations of some properties in all animals could be attained, from an investigation of them in a few, and it will appear that *his* example does not give a fair view of the entire character of the inductive process.

It may be inferred, therefore, that some distinctions should be drawn as to the methods of inductive procedure; and we shall now attempt to classify, and define, at least the more prominent modifications. These seem to be nearly as follows:—1. There is a form of procedure, in which, in order that a law may be expressed with logical precision, or possess physical certainty, it is absolutely necessary for every individual instance included in the original statement to be examined. Thus, in systematic Botany, when the external conformation of a class of plants has been described, and it is found, that, in some plants of that class, the particular configuration of the family exists along with a peculiar medicinal or culinary virtue; it yet cannot be stated, on the grounds of such a knowledge, what may be the virtues of other plants of the order. A presumption may be formed from the investigated cases, as to the nature of the relations of certain properties in the individuals not examined; but it must be a mere hypothesis. If, however, the botanist had the power of experimenting upon a cruciferous plant, so that he could find the four crosswise-placed petals and the peculiar shaped pod to be essentially connected with the virtues of a plant,—in such a manner, as that while the structure was modified, the other property should also undergo modification,—and in fact so that a certain not-to-be-doubted relation could be detected between the two sets of properties,—he might then conclude, on the conviction of the uniform order of nature derived from past experience, what were the virtues of all cruciferous plants. But he cannot perform such a precise experiment, and therefore must be content to collect every instance, before drawing a general conclusion. 2. There seems to be a class of sciences, of which we shall take Medicine as an example, in which most of the inductions are defective; or, more properly, where there cannot be formed in most cases any complete induction, as in all such cases logically defined genera cannot be procured, since new instances are being continually created,—beings with diverse constitutions; so that from the examination of already existing cases, no perfectly certain inference can be drawn as to the whole class of analogous cases; and where, besides, the differences between individuals are such as to prevent one individual being taken as a sufficient example of another; so that the induction can only conclude with certainty as to an individual. And the history of medicine affords proof of the enormous difficulty thus opposing generalization. Suppose that the diseased structure of the intestinal glands could be found in one case to preserve a constant relation with the symptoms of fever; yet this invariableness of accompaniment should only afford a presumption as to what may exist in other cases; for, there can be no conviction in the uniformity of nature, when the actions of nature in each individual are known to differ.* 3. In this present division the groups of facts are arranged in genera, each of which is not illimitable throughout the known universe, but confined, and finding some similar, and many closely analogous to itself, so that the law framed of one group can easily be transferred to another; while in each group, the investigation is capable of being abridged by means of prerogative instances. By way of illustration, we shall take the doctrine of the circulation of the blood, introduced by Harvey, which at the same time will give a good example of the course of inductive procedure ordinarily pursued in the physical sciences. If, then, we take that celebrated doctrine, and spread it out, so as to display all its parts, and ask proof for every assertion made in it, we should demand such a knowledge of the structure of the heart and arteries as to be sure of their powers and capabilities to allow of the course alleged, and to perform the functions ascribed to them,—evidence that the heart sends the blood into the aorta, like evidence of it being sent along the arteries into the minute veins, of the return, and the same kind of proof of the lesser circulation as of the greater. It is believed that when Harvey announced the circulation, he was not able to furnish all of those proofs, and, in particular, that he had not evidence of the actual passage of the blood from the small arteries into the small veins. Of the lesser circulation he probably could only offer the analogy of the parts performing it with those concerned in the greater. Yet his doctrines, founded on the proof he gave, must be acknowledged possessed of such evidence, as that, if more be added, it can only amplify the notion he gives of the circulation. Two grand facts are the proofs of this great theory: 1, the prerogative fact of the valves of the heart and veins; and, 2, the analogy of the parts engaged in the lesser circulation with those that perform the greater. But the adaptation of the valves to their function is the grand proof of the whole theory. Now, the theory of the circulation was proved originally on deer; and the extensive analogies, which in fact are but covert similarities, traceable throughout the animal kingdom, allow of physiological doctrines being transferred readily from one genus to another. Many inductive doctrines in Chemistry resemble very closely, or rather exactly, the law of the circulation, both in their original frame-work, and in the mode of transference they admit of to other genera apparently only analogous but essentially similar. 4. There is another modified form of induction, the most definite of all, which may be described as follows:—Here each law is not confined, as in the third species, to a single group of a few facts, nor do any exactly corresponding groups exist, to which the law when framed is applicable. But each law extends throughout the known universe, and although the class of facts to which it applies may, or rather must, be defined, the number of instances is illimitable. Each law, however, can be framed from the examination of a few prerogative cases. And as in this kind of inductive generalization, the laws themselves form again parts of a mightier whole, which is framed from them in much the same manner as they themselves from their facts, at length an axiomatic expression is reached, which, arrived at from the investigation of a very small number of instances comparatively, yet includes in its expression an immense array. Illustrative examples exist in Dr Well's theory of dew, and the laws of gravity. We are aware, that all those different forms of inductive procedure agree in kind, except, perhaps, the one particularised as practised in medical science; and also, that the divisions between them, however carefully drawn, are exceedingly nice, and perhaps such as, by a close analysis, might be found to disappear. Indeed, the mind gains all its inductive knowledge by one process viewed in connexion with its own functions, and that is by complete proof; the various steps of which we have endeavoured to relate as fully as the vast extent of the subject would permit within moderate limits. Complete enumeration of all the instances composing a law, is that degree of proof which would be always essential, were it not for the indices supplied by prerogative facts. Indeed, we may regard all those forms of inductive procedure but the third variety, as derived from that simple form, by the introduction of prerogative facts within the spheres of the different genera existing in nature. Thus, as the science becomes more elevated and complicated, there is the greater power possessed of arriving at extensive inferences by means of well related facts. But it must be borne in mind, that the prerogative facts are those needing most the resources of experiment and calculus, in order to make them known so as to be of use in drawing inductive inferences. Residual facts are those left in a genus, uninvestigated, or rather partially explored, in order that the prerogative facts may be studied; and that property of physical laws called anticipation, is nothing more than the power possessed in some cases of abridging inductions by means of the prerogative facts.

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* Brown's Lectures, vol. 1. sect. 8. p. 176.

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* A paper was read before the members of the Royal Medical Society, on the 6th of April last; the object of which was to prove, that in medical reasoning, the only constant source of uncertainty is in idiosyncracy. There may be great complexity in the relations of properties—great difficulty and perplexity in the investigation; but the only constant source of uncertainty arises from individual peculiarities. See Dr Abercrombie on Certain and Uncertain Sciences; he classes together medicine, political economy, and ethics.

Thus, the great property of those prerogative facts is, that they admit of being experimented on, being indeed, when completely known, exactly similar to the results of experiment; since they then afford a view of the relations of the properties composing them in very varied circumstances; it follows, therefore, that to term a science an experimental science, is just as if we were to say, that it abounds in prerogative facts. So that if the science of mind be, or be not, an experimental science, the question can best be determined by seeing whether its facts admit of being classified into some such heads, as the prerogative instances of Bacon, contained in the Novum Organon.

Many applications might be made of the observations in this essay, if these latter be founded on truth. In particular, the history of science might perhaps be elucidated still further through their means.

Note.—It is proposed to term the forms of induction described above in their order. 1. Simple. 2. Enumerative. 3. Prerogative; and 4. Complex. The following formulas will express the simple and prerogative forms. 1. Of the Simple form. Let there be any number of instances, as $n\,A$; and of these some are found possessing the property a; before it can be said that this a exists in all $n\,A$, they must be all examined; and also the same must be the case should a be found connected with the property b in any of $n\,A$, before it could be said that $a + b$ exists in all $n\,A$. And if a, or $a + b$, should not be found in $m\,A$; then the law states that $\overline{n-m}\,A$ contain a or $a+b$. 2. Of the Prerogative form. Properly speaking we should start here with the knowledge of the existence of the property a in $n\,A$, or $\overline{n-m}\,A$; let $n\,A$ then contain a; here, instead of seeking to enumerate every instance before obtaining a law for the class, it is enough to know that in A', A'', or A''',—the prerogative instances, a is connected in a manner believed essential with b, or $b+c$, or $b+c+d$ in order to frame the law—"All $n\,A$ contain $a+b$, or $a+b+c$, or $a+b+c+d$." One sufficient prerogative instance can enable a negative conclusion to be drawn, just as in the case of a positive law. The downward application of laws framed from the study of prerogative facts furnishes the means of verifying the observations already made.

We are informed by an eminent authority in logical science, who honours these pages with his general approval, that Duns Scotus distinguished two species of induction, corresponding with our first and second forms. Bacon confounded the induction given by way of example by Aristotle, and which therefore was our first form, with the second or uncertain form practised in medicine.* The great improvements effected by Bacon on the views of his predecessors with regard to induction, consisted in the extensive grasp he took of the province in the cultivation of science appropriated to induction; and also in shewing the power possessed by particular well chosen facts. These, the most important and original of his notions have yet received a too *implicit* attention from those who have written on his philosophy. Until about the epoch of the Novum Organon, philosophers were not in possession of such instruments as are requisite to make known fully the relations of most of those instances termed prerogative. Till then, therefore, induction was described as a mode of mental procedure; Bacon described it by its corresponding signs in nature. Therefore it was, that his precepts were so powerful in displaying the advantages attendant upon the cultivation of science, inductively.

Organic Remains in the Old Red Sandstone of Fife. By the Reverend JOHN ANDERSON, Minister of Newburgh. Communicated by the Author.

THE county of Fife occupies the eastern portion of the great independent coal formation of Scotland, bounded on the south by the Frith of Forth, on the east by the German Ocean and Bay of St Andrews, and on the north by the river Tay. It may be regarded as divided into three principal geological districts; that of the coal measures, which occupy exclusively the southern district, lying betwixt the Forth and the Lomond range, as it stretches towards St Andrews; that of the yellow sandstone, which occupies the valley of Stratheden; and that of the old red sandstone, which skirts the northern escarpment of the Ochils, and which prevails throughout the lower basin of the Tay.

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* Nov. Org. P. 1, s. 6, Aph. 105.

Dr Fleming, in 1830, read before the Wernerian Society a notice " On the occurrence of Scales of vertebrated Animals in the Old Red Sandstone of Fifeshire." These organisms, as described by him, occurred in the yellow sandstone of Drumdryan, and the grey sandstone of Parkhill. From the former locality scales of a fish were obtained, and from the latter impressions of gramineous vegetables, referred by the author to an extinct species of the genus Juncus or Sparganium. The same paper contains a notice of the occurrence of similar scales in the old red sandstone of Clashbennie, near Errol, in Perthshire, one of which is described as bearing " a very close resemblance to some of the scales on the common sturgeon, and may, with some probability, be referred to an extinct species of the genus Acipenser." Also a portion of a fish, about seven inches long, two inches deep, and from seven to eight-tenths in thickness, is noticed as having been obtained from the same quarry, and which the author considers as " probably identical with the *Dipterus macropygopterus* of the Caithness beds."

In the new work of Agassiz " Sur les Poisson Fossiles," these relics are noticed, and the scales are referred to a species of the Gyrolepis. Agassiz describes four species which belong to this genus, viz. the Gyrolepis Albertii, G. tenuistriatus, G. maximus, and G. giganteus. The scales, both of Drumdryan and Clashbennie, he considers as belonging to the species *giganteus*, of which he thus speaks: " Ce sont les plus grandes écailles de poissons que je connaisse; elles ont souvent plus de deux pouces de diamètre; mais leur épaisseur n'est pas proportionnée à leur grandeur, car elle n'excede guére trois lignes. La partie de ces écailles qui n'etait pas recouverte par leur imbrication est sillonnée de rides profondes et treslarges, qui presentent de fréquentes anastomoses, et qui sont généralement dirigées obliquement d'avant en arriére. La partie de l'ecaille cachée par la série antérieure égale environ le quart de la longeur totale; ella est completement lisse. Tous ces caractéres," he adds, "se rapprochent assez de ceux qui ont été assignés aux Gyrolepis du terrain *triasique*, pour j'aie cru pouvoir ranger ces écailles dans le même genre. Je ne connais non plus encore du G. *giganteus* que des écailles détachées."—*Tom.* ii. p. 175.

Important, then, as these organisms must, in every view, be considered, I have much pleasure in communicating some additional particulars respecting them, having, in the course of last autumn, found the scales in three other localities in the county of Fife, besides those noticed above. Indeed, they are the only instances in which such organic remains have been detected in the old red sandstone within the limits of the county, as Dr Fleming's paper refers solely to the scales found at Drumdryan, which is the yellow sandstone, and at Clashbennie, which is in Perthshire. The vegetable impressions which he mentions as occurring in the grey sandstone of Parkhill, I have likewise detected in several other localities.

1. *Old Red Sandstone.*—While the valley of Stratheden has been referred to as characterised by the yellow sandstone deposit, yet it is not exclusively so, as the old red likewise occupies a portion of the district. It occurs in the parish of Dairsie, on the north bank of the Eden, where a quarry has been opened to the eastward of the church, and where the dip of the rock is to the S. E. at an angle of 15°. There I found these remains in the greatest abundance, and of the same characters and dimensions as those of Clashbennie. The texture and colour of the two deposits are likewise perfectly identical, so much so, that having accidentally mixed some specimens obtained at Dairsie with those brought from Clashbennie, I am now unable to distinguish them from each other. This bed ranges eastward, and occupies the entire bottom of the valley from this point as far as the Guard Bridge, where it abruptly terminates. Drumdryan quarry is situated in the ridge to the south, which is wholly composed of yellow sandstone, rising in many places to the height of 600 feet. Dairsie quarry, containing the scales, is a lower member of the series, separated, however, as will immediately appear, by the interposed bed of limestone which occurs at Craigfoodie.

Birkhill, the next locality where the scales occur, lies upon the north declivity of the Ochils, and south bank of the Tay. Here the sandstone rises immediately from the bed of the river, and is very confusedly mixed up with the trappean rocks. Very little of the deposit is exposed to view, although in proceeding along the beach towards the pier of Balmerino, it may be observed cropping out in various places, and dipping towards the S. E. It appears to be somewhat variegated, containing considerable portions of whitish rock, and approaches also in some parts to the character of a conglomerate. In this locality I have only been able to obtain one specimen of a scale, which, although of the small dimensions of half an inch in length, and about a quarter of an inch in breadth, possesses all the characters of a perfect organism, evidently belonging to the same class as those of Clashbennie, Drumdryan, Dairsie.

The colour and quality of the soil to the westward, throughout the barony of Balambriech, afford a pretty decisive test of the continuation of the old red sandstone deposit, but there is no outcrop till we reach Parkhill, near the monastery of Lindores. Here I first detected the scales in the stones which form the embankment and breakwater pier along the side of the river. At first I suspected that the materials of which these are built might have been brought from Clashbennie, which lies adjacent on the opposite bank of the Tay, and in this opinion I was the more confirmed by the perfect similarity of the scales, as well as from the texture and colour of the stones. But in this I was speedily undeceived upon proceeding to the quarry itself, which lies a few hundred yards to the south, and near the eastern extremity of the farm of Parkhill, where, amongst the *rede* or debris, I found these organisms in the greatest abundance. The quarry is now so completely covered in that no portion of the rock can be observed, but a little to the westward it again emerges under a bed of coarse limestone, and resting upon a mass of greenstone trap, through the agency of which, doubtless, it has been here brought to the surface. The materials of which the monastery was constructed were obtained from the quarry, as appears from a charter of the founder, granting the use of it "to his monks there serving God;" but, as there were no geologists in those days, these interesting relics have found no chronicle to record their history, although portions of them may still be detected among the ruins of the building, as well of the ancient castle of Balambriech, which stands on the promontory about a mile to the eastward of the quarry.

This rock again crops out to the surface in a narrow glen a little to the south of Abernethy, where the line of dip is altered from the S. E. to the N. W. It lies upon a disturbing mass of trap-tuffa, which has thrown it into a very inclined position, and considerably indurated the texture of it. It is the yellow spotted variety, similar to that which occurs at Strathmiglo, on the opposite side of the Ochills, and is a portion, I have no doubt, of the same bed. From this point towards Dunning it emerges in various places, sometimes assuming the character of a coarse conglomerate, and at other times that of a remarkably fine-grained compact rock, which is much used in building, and having a deep hematite red colour. Hitherto I have not been successful in detecting any organic remains in this district, although a notice appeared in the Perth Constitutional newspaper, announcing that scales had been found in the Hilton quarry. But beautiful dendritic forms occur abundantly in some of the fine-grained beds, particularly at Dumbarnie and Pitcaithley, and which had been rather hastily assumed by some geologists as exclusively characteristic of the new red sandstone.

2. *Limestone or Cornstone.*—In determining the position of the deposit which contains the scales in the several localities noticed above, it will be of consequence to retrace our steps for a little, in order to examine more particularly the limestone which accompanies it, and which it immediately underlies.

The mountain limestone, full of organic remains, may be observed at the Lomonds, and also at Cults, reposing upon the yellow sandstone of which Drumdryan is a continuation, and immediately below the yellow sandstone lies the calcareous bed of Craigfoodie, a concretionary mass destitute of all traces of organic remains. The Cornstone bed next occurs at Newton, on the very summit of the Ochils, where it is completely insulated by the trap from the sandstone, which on both sides of the range crops out at Strathmiglo and Abernethy about 600 feet beneath. At Parkhill the deposit rests immediately upon the old red sandstone which contains the scales, and underlies the yellow sandstone, where the calcareous and arenaceous beds insensibly pass into one another. A bed of limestone also occurs in Strathearn in the same relative position to the old red sandstone in the parish of Forgandenny, and was detected a few years ago on the farm of Little Kinnaird by the proprietor Laurence Oliphant, Esq.; in consequence of the excavation of some drains. On the north bank of the Tay a similar deposit occurs at Meurie, likewise at Ballendean, and also on the north side of the Sidlaws, in the parish of Cargill.

In all these places the quality of the limestone is nearly the same, and its position, in every locality, is determined by its relation to the uniformly associated bed of red sandstone on which it rests. No organic remains—not even the smallest indication of them—have been detected in any portion of it, while the calcareous is so mixed with arenaceous matter that it has been but sparingly used for either building or agricultural purposes. The most solid portions of it are compact or sub-crystalline, and are generally of a yellowish-green or grey colour. The structure is concretionary, and when the softer parts are washed away, the rock assumes a conglomerate or brecciated appearance. The Parkhill bed contains more calcareous matter, and is more compact than any of the rest. Some portions of it are cherty containing chalcedonic veins, and small globular cells, which are coated over with mammillated, reddish chalcedony; but, generally, it may be described as a compact concretionary deposit, with several interposed beds of a green and red pyritous marl, and stained on the surface with innumerable dendritic figures.

Dr Thomson, in the new edition of his Chemistry, recently published, considers the beds of sandstone which traverse Strathearn and the Carse of Gowrie, from their great horizontality, as belonging to the *new red.* What is the inference necessarily deducible from the above statement of facts? The Dairsie sandstone and its accompanying limestone at Craigfoodie dip under the yellow sandstone, which constitutes the lowest member of the coal formation. This therefore must be the old red. But the sandstone which occur in the Strathearn and the Carse of Gowrie districts are similar in respect to position, organic contents, and mineralogical characters, and hence they must be regarded as belonging to the same geological epoch. Dr Fleming, in the paper referred to above, considers that there are two beds of limestone inferior to the mountain limestone in Fife, the one occurring towards " the lowest part of the yellow sandstone," and the other " towards the upper part of the old red, nearly similar to the one which has been mentioned as existing at the lower part of the yellow sandstone." But for this distinction I can see no sufficient reason ; the elevated, insular position of the Newton bed renders it difficult, indeed, to trace the relation to the other members of the series, while in its structure it approaches nearer to a conglomerate than any of the rest ; but it partakes of this character in common with that of the Cargill limestone, which rests immediately upon the old red sandstone. And thus there appears to be but one deposit, coextensive nearly with the old red sandstone, passing like that rock from the homogeneous to the conglomerate state, and of a mixed inferior quality throughout.

3. *Grey Sandstone.*—This deposit I consider as underlying the old red sandstone, being the next member of the series in the descending order. It is but very sparingly distributed in Fife, and is wholly confined to the northern extremity of it. Proceeding along the south bank of the Tay, from east to west, the grey sandstone first appears at Wormit Bay, cropping out from the bed of the river and dipping to the SE. at an angle of 32°. It alternates with claystone, compact felspar, and amygdaloid, when it speedily changes the dip to SW. at an angle of 22°, and becomes so interlaced with these rocks as to split up into thin beds of half an inch to a quarter in thickness. These beds are generally of a bluish-grey colour, closs-grained, having a tendency to exfoliate parallel to the laminæ of deposit, which are highly micaceous. In some place, however, they run into one another, so that it is almost impossible to determine where the depository bed ends, and the amorphous or crystalline rock begins. The original inclination is again resumed as we follow the direction of sandstone westward through the parish of Balmerino, (where it is used for building materials as well as roofing) until it terminates on the east escarpment of Norman's

Law, at an elevation, as compared with its emergence from the bed of the river, of about 700 feet. The texture of this rock is finer and more compact than the red variety, consists essentially of clay with very little siliceous matter, and contains a considerable quantity of mica, which causes it to decompose in thin plates. There are very few nodules of either clay or quartz in it.

Towards the lower part of this rock, there are two beds of a slaty clay, one of which is more highly indurated than the other, and of a lighter colour. These may be observed upon the beach at Wormit Bay, near the fisher's lodge, the dark-coloured at once attracting the eye by its coaly aspect. Both of them are very friable, and softened into an unctuous clay by the constant action of the tide-wave. The light-coloured bed contains more mica than the other, and is divisible into thin laminæ, between which the flattened stems of vegetables occur in the greatest abundance. Numerous circular forms, from half an inch to about an inch in diameter, are intermixed with the minute gramineous culms, but in no instance have I found them connected so as to form an entire plant. The bed at Parkhill, about ten miles to the westward, differs very little from that which has been described, except in its fossil and slaty structure, bluish colour, and greater hardness. It is, in all probability, a continuation of the same, as the vegetable impressions, in both localities, seem to be identical in their specific characters. "These occur," says Dr Fleming, "in the form of circular flat patches, not equalling an inch in diameter, and composed of numerous equally contiguous circular pieces, not] unlike what might be expected to result from a compressed berry, such as the bramble and the rasp."

Differing from Dr Fleming as to the position of the grey sandstone, which he places *above* the old red rock, I shall state a few of the localities from which I have obtained specimens containing the same vegetable impressions, and which clearly warrant the inference as to its *inferiority* in the order of superposition.

The colour and texture of the Wormit Bay grey sandstone very closely resemble those of the Kingoodie rock, so extensively used in building, and which occupies a corresponding position on the opposite bank of the Tay. One specimen, with organisms of precisely the same character with those of the gramineous vegetables of the Wormit Bay and Parkhill deposit, is now in my possession. The impressions which are so abundant in the Carmylie beds are well known, and these, as well as Kingoodie, are universally admitted to be inferior to the old red, or rather are regarded as the lowest members of one great formation, of which the old red variety is the most distinguishing type. I have impressions, likewise, from the grey sandstone which extends from Dunkeld towards Murthly Castle, and also specimens of the same from two localities in Strathearn, namely, the Bridge of Forteviot, and the village of Dunning, at both which places the *grey* unquestionably underlies the *red* deposit. From this point on the northern slope of the Ochils towards the Grampians, and eastward throughout the entire valley of Strathmore, the grey sandstone is the prevailing rock. Underneath are clay-slate and greywacke, which observe the same general line of bearing, resting upon the primitive rocks of the Grampian chain, and occupying a tract of considerable extent along their southern acclivities.

The whole of the district thus cursorily noticed, deserves the attention of the geologist, not only from its comprehending, within a comparatively limited and very accessible space, the various members of the coal measures, as well as the mutual and diversified relations of the carboniferous rocks, and those of the sandstones below them; but also from the abundance of organic remains which the latter are now found to yield in almost every locality, and which there is reason to believe, from the disclosures of every day, are both more numerous and diversified than could have been anticipated. We have traced four separate and well-marked deposits, all inferior to the mountain limestone of the coal-field. 1. The yellow sandstone of Drumdryan, which, in addition to the scales already noticed, contains the bones, teeth, and palates, in the greatest abundance, of some extinct species of the ancient world. 2. Patches of a coarse arenaceous limestone, destitute of fossil remains, and conformable in dip, bearing, and extent with the inferior sandstone formation. 3. The old red sandstone, which, like the yellow variety, yields abundantly organic remains, at Dairsie, Birkhill, Parkhill, and Clashbennie, resembling, if not essentially the same with, those which occur at Drumdryan. 4. The grey sandstone, which is the lowest member of the series, and which is distinctly characterised in Fife, Strathearn, and Forfar districts by the remains of those culmiferous vegetables, which are so numerously distributed throughout the deposits.

On the Fossil Organic Remains found in the Coal Formation at Wardie, near Newhaven. By ROBERT PATERSON, M.D. Member of the Wernerian Natural History Society of Edinburgh, &c.* (Communicated by the Author.)

IT is well known to geologists, that the coal formation in Scotland is situtated in the great valley of the Scottish lowlands, separating the primitive country of the north of Scotland from the transition series of the southern border; and that a line drawn from the mouth of the Tay, through Stirling to the north extremity of the Island of Arran, and another nearly parallel to it, from St Abb's Head on the east coast to Girvan on the west, will include between them almost the whole of the coal-fields in Scotland, with the exception of the basins in the Nith and Esk in Dumfriesshire.

The strata, however, to which I now particularly wish to direct the attention of the Society, are situated at Wardie, on the south coast of the Firth of Forth, about two and a half miles north-east of Edinburgh, and a little beyond the village of Newhaven. The sea and weather, acting upon the rocks in this place, have exposed them to view, and they exhibit very characteristically the strata of the coal formation. The strata are slate-clay, bituminous shale, sandstone, fire-clay, clay ironstone, with beds of black bituminous coal of no great thickness. They in general dip to the south-east, excepting where they appear to have been changed from their original position by some after action.

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* Read before the Wernerian Natural History Society, in November and December 1836.

Many interesting geological phenomena are seen in this beautiful section, which we cannot notice at present, as we purpose confining our attention chiefly to the imbedded fossil organic remains. We shall first describe the fossil plants, and afterwards notice the fossil remains of animals.

1. *Fossil Plants.*

They generally occur in the form of impressions, more rarely in that of casts, the latter being frequently composed of a centre of iron-pyrites, with a surface of bituminous matter, while in others they are entirely formed of a substance resembling jet, the external part of which is of a soft texture, bearing, however, the different vegetable markings distinct upon it.

In this place, like the other coal-fields which have been examined, the majority of the vegetable remains may be referred to the tribe of Ferns. Brongniart, indeed, estimates that two-thirds of the coal formation flora, belong to this class of plants. The species of Filices may be referred to three genera, viz. *Sphenopteris*, *Cyclopteris*, and *Neuropteris*.

I. Genus *Sphenopteris*. The most abundant species of this genus is the *affinis*; it occurs almost universally disseminated through the shale, not being confined to any particular stratum; the best specimens, however, are always to be found at the point where a thin layer of clay ironstone joins the shale. The *Sphenopteris crythmifolia*, *S. artemisiæfolia*, *S. furcata*, *S. elegans*, *S. Hoeninghausi*, are also occasionally found, but in more sparing quantities than the *affinis*. Of the four first in order of these species, it may be remarked that they bear a great resemblance to each other, some authors, indeed, classifying them all under one head, the *affinis*.

Although it is difficult to distinguish them, the determination may be made by a minute inspection of their nervures, and by reference to good plates, as those in the works of Brongniart, and the British Fossil Flora by Messrs Lindley and Hutton.

II. Genus *Cyclopteris*. This genus, which is the next in point of abundance, equals, if it does not surpass, the *sphenop-*

its yet retaining great thickness, at least for a fossil vegetable. Although Messrs Lindley and Hutton are inclined to believe that their fossil was a fungus, they admit that certain objections might be brought against it. "These consist in the lines in the spaces where the surface is broken off not being in accordance with the radial lines near the margin; this, however, may have been caused by the pressure of another fungus lying in a somewhat different direction, or that the pileus was two or three layers in thickness." It appears more probable, however, that their first explanation is most satisfactory, and that there was only one pileus; that the want of the radial lines in their specimens and in ours, which seem to be precisely similar in the small portions referred to, is in consequence of the pileus being removed and retained on the opposite side of the shale. That it was cellular on its lower surface, and probably throughout like other polypori, appears to me, first, from the appearance of the detached portions of the surface as before mentioned; and, secondly, from the difficulty, nay, impossibility, to separate the lower surface of the fossil from the shale in which it is contained. This must be in consequence of the shale, when in a fluid state, entering the pores on the lower surface, and thus becoming completely incorporated with the fossil as the deposition proceeded.

Knorria. The next specimen I exhibit may be referred to the *Knorria taxina* of fossil botanists; it bears the termination of a branch which is very similar to the fruit-bearing branches of our common yew. *Sphærida paradoxa*, *Poacites cocoina*, *Antholites Pitcairniæ*, species of *Bechera* undetermined, and *Fucoides Targionii*, sum up the list of fossil vegetables. There are many, however, which we have not been able to name, but which we hope to be able to communicate at a future time.

2. *Fossil Remains of Animals.*

In turning from the vegetable to the animal remains of this locality, we cannot commence with any which are more interesting, or which have of late attracted more notice than the Entomostraca, the external coverings of which, of a white egg-shell appearance, contrast beautifully with the dark-coloured carbonaceous slate in which they occur.

The Entomostraca which are found in this place, are to be referred to the genera Cypris and Daphnoidea; the former of which occur in greatest abundance in some of the shelves of the rock, even giving it a greyish tint of colour, and oolitic appearance.

These interesting fossils are always found associated with terrestrial plants; and in consequence Dr Hibbert has been led to consider the rock in which they occur as of lacustrine origin.

In concluding his memoir on the so-called lacustrine deposit at Burdiehouse, he says—

"The circumstantial evidence is to the following effect, that the calcareous deposit of Burdiehouse must have taken place in a depression or basin perfectly surrounded with a dense vegetation, which has been washed into inland water. But this circumstance would of itself prove little, as we may easily suppose that an estuary or arm of the sea might have stretched through a tract where a dense vegetation has prevailed; but when, in connexion with a perfect absence of all acknowledged marine remains whatever, we find plants enclosed in a calcareous deposit in the greatest perfection imaginable, what conclusion remains but that such a deposit is more indicative of a fresh water river or lake, than of a sea or estuary?

"That while the inland waters which deposited this limestone were actually unfavourable to the existence in it of acknowledged marine mollusca or conchifera, they were not unfavourable to countless myriads of Entomostraca, one genus of which, the Cypris, is the recent inhabitant of fresh-water marshes; and it may be fairly suspected that other genera associated with it were equally so.

"In short," says he, "the evidence in a general point of view, leads to the following conclusions: There are, it is well known, numerous deposits of limestone belonging to the carboniferous group of rocks, in which, to the exclusion of any vegetables of a Tropical flora, nothing but marine products, such as corallines, or acknowledged shells of genera hitherto found in open seas, only have been discovered. With a calcareous deposit of this kind, the comparison of another destitute of all corallines or marine shells whatever, yet containing, in an abundance perfectly remarkable, the plants of Tropical marshes along the Entomostraca of marsh waters, can surely lead to no other conclusion but the following: That, while the first formations must have taken place in a pelagic bed analogous to one of recent times, the other must have been the result of a fresh-water deposit, which, while it was no less hostile to the growth and increase of marine shells or corallines, must have flowed through marshy tracts wherein grew all the plants observed in our coal-fields."

Professor Buckland, in his late Bridgewater Treatise, appears to adopt this opinion, for he says, in a note to page 275,—

"The limestone (viz. Burdiehouse) in which these fishes and coprolites occur, is also abundantly charged with ferns and other plants of the coal formation, and with the crustaceous remains of cypris, a genus known only as an inhabitant of fresh water.

"These circumstances and the absence of corals and encrinites, and of all species of marine shells, render it probable that this deposit was formed in a fresh-water lake or estuary; it has been recognised in various and distant places, at the bottom of the carboniferous strata near Edinburgh."

But while some individuals seem thus to adopt the opinion of Dr Hibbert, there are not a few who have been all along satisfied that the limestone of Burdiehouse was no other than the common one of the coal formation. This opinion was formed not from any contradictory evidence with regard to fossils, but chiefly from the geological relations of that rock to the coal series in the immediate neighbourhood.

These microscopic crustaceous remains occur at Wardie, chiefly in the slate, and are to be seen in conjunction with a small shell of the genus *Ostrea*, with *corallines*, with plants to be referred to the genus Fucoides, as well as with the multitude of terrestrial plants.

Dr Hibbert's arguments are chiefly based on the vast abundance of these microscopic Entomostraca, together with the absence of marine remains; but certainly these assumptions do not afford sufficient grounds for asserting, that the deposition took place in a fresh-water lake.

The occurrence of fresh-water strata alternately with marine in the common coal-fields, has long been known, and was attributed to the varying relative abundance of fresh or salt water: some of these fresh-water strata might have been of considerable thickness, and yet few would have supposed them to have been lacustrine. The strata in this place, however, distinctly shew the unsatisfactory nature of the arguments which have been brought forward to prove the lacustrine origin of the Burdiehouse and other limestones in this neighbourhood, because they contain microscopic entomostraca in conjunction with corallines and shells of the genus Ostrea, as well as with abundance of terrestrial plants, and genera of fish similar to those of the supposed lacustrine formations. From these facts it is evident that the strata here have been deposited in an estuary.

The fishes which have been noticed here may all be referred to the following genera: *Amblypterus*, *Palæoniscus*, *Eurynotus*, *Acanthodes*, and *Pygopterus* of M. Agassiz.

Genus I. *Amblypterus*. Three species are found here, viz. *Amblypterus striatus*, *nemopterus*, and *punctatus*. Of these three species, the most common is certainly the *punctatus*, and this accords with the opinion of Lord Greenock and M. Agassiz. The *striatus* may be mentioned as next in point of abundance, and it constitutes the majority of those fish found in a disjointed condition.

The specimen which I now exhibit to the Society, is also to be referred to the genus Amblypterus, but to no species which I have as yet seen described. It is about four inches long, and very broad around the thorax. The head seems to have been very large in comparison to the size of the fish, the body of which tapers gradually till it reaches the base of the tail, which is very small. The tail itself, as well as all the fins, are of great size, and this, in my opinion, constitutes the chief distinguishing marks of this species. So large, indeed, is the caudal fin, as to give the tail quite a symmetric appearance, as will be seen in the present specimen. The anal fin is also very long, more especially at its anterior position. The fins are composed of pretty coarse rays, the divisions of which are considerably separated. It is not at all common in this place.

II. Genus *Palæoniscus*. Of this genus there is only one species which I have noticed, viz. the *striolatus*, and it in general occurs very well preserved.

Genus III. *Eurynotus* presents us with the most beautiful and perfect examples of fossil fish which I have as yet seen preserved; and another circumstance which adds greatly to their interest is the species to which I allude being quite characteristic of the locality. This genus was a new one to M. Agassiz when he came to Scotland, and it constitutes a connecting link between the *Platysomus* and *Amblypterus*. The *Eurynotus fimbriatus* is figured by the above named author from Wardie. Unfortunately, however, he does not seem to have had specimens exhibiting perfect scales. There is no difficulty in referring the present specimens to that species.

There is a species of this fish, however, which it may be right to make particular mention of, from the manner in which it lies in the stone, a position which of course it must have had when enclosed. It is compressed from above downwards, consequently pressed out laterally. In looking at the nodule, as it is broken in the usual horizontal manner, the jaw, set with minute conical obtuse teeth, is distinctly seen, as well as the pectoral fins; and again, on looking at a perpendicular section of the same stone, the dorsal, ventral, anal, and caudal fins are all observed. From the enumeration of the parts to be seen in this single specimen, it will at once strike us how peculiarly well this position is calculated to display every part of the fish which has the slightest interest; at the same time that it shews us, that the animal was most likely enclosed suddenly in the act of swimming, and in the very position in which it must have swam.

Genus IV. *Acanthodes.* The *Acanthodes sulcatus* presents us with beautifully perfect specimens. The one which I have now the honour of shewing to the Society measures about one foot six inches in length; unfortunately, however, the fins are entirely wanting, but in another and smaller specimen, the pectoral fins are to be seen of considerable size.

Genus V. *Pygopterus.* With a specimen to be referred to Pygopterus, we conclude the list of the fossil fishes of this place, which must certainly be allowed to be rich in these remains.

Coprolite.—We may add, that coprolites or fœcal balls abound in the slate and clay ironstone; and here, as in the different coalfields in the middle district of Scotland, contain scales and teeth of fishes.

Concluding Remarks.—It is now almost universally admitted, that coal has resulted from the distribution of vegetable remains over areas of greater or less extent upon a previously deposited surface of sand, or argillaceous silt, and that, after the entombment of this mass of vegetable matter, other layers of mud and clays were deposited on it; and this operation must have continued for a considerable length of time, during which it is more than probable that an abundant vegetation existed at no great distance from the spot where this process was taking place.

These changes are most likely to have gone on in estuaries or indentations of the land, into which this mass of vegetable matter was brought by rivers, while the sea made occasional, or in some instances continuous incursions. In this manner we can easily see how, by accidental circumstances, the productions of the land or sea in greater or less abundance are occasionally met with in the strata of the carboniferous series, and how in one coal district an examination of its organic remains may afford one individual reason to believe, that the strata were chiefly deposited in salt water; a second, from the examination of a different locality, that they are entirely fluviatile, or probably he may imagine them lacustrine; while a third may see the same organic remains which have given rise to the two former opinions so mixed up and incorporated together in another place, as to leave no doubt on his mind that the two former cases were only extreme degrees of what he had noticed.

The examination of the strata in this place, then, is well calculated to clear up any prejudiced notions which we may have formed on the subject of the formation of coal strata; for the organic remains, and their relative positions which we have mentioned, go far in shewing the very frequent alternations, nay, admixtures of the same kind of organic remains in the same rock, which, when taken singly, has led to such contrariety of opinions.

On the Account of the Creation in the First Chapter of Genesis.

By Charles Babbage, Esq., F. R. S. E.

A strange and singular argument has frequently been brought against the truth of the facts presented to us by geology,—facts which every instructed person may confirm by the evidence of his senses. It has been stated that they cannot be true, because, if admitted, they lead inevitably to the conclusion, that the Earth has existed for an enormous period, extending perhaps, over millions of years; whereas, it was supposed, from the history of the creation, as delivered by Moses, that the Earth was first created about six thousand years ago.

A different interpretation has been lately put upon that passage of the sacred writings, and, according to the highest authorities of the present time, it was not the intention of the writer of the book of Genesis to assign this date to the creation of our globe, but only to that of its most favoured inhabitants.

Now, it is obvious that additional observations, and another advance in science, may at no distant period render necessary another interpretation of the Mosaic narrative; and this again, at a more remote time, may be superseded by one more in accordance with the existing knowledge of that day. And thus the authority of Scripture will be gradually undermined by the weak, though well intentioned efforts of its friends in its support. For it is clear that when a work, translated by persons most highly instructed in its language, and seeking in plainness and sincerity to understand its true meaning, admits of such discordant interpretations, it can have little authority as a history of the past, or a guide to the future.

It is time, therefore, to examine this question by another light, and to point out to those who support what is called the literal interpretation of Scripture, the precipice to which their doctrines, if true, would inevitably lead, and to shew, not by the glimmerings of elaborate criticism, but by the plainest principles of common sense, that there exists no such fatal collision between the words of Scripture and the facts of Nature.

And first, let us examine what must of necessity be the conclusion of any candid mind from the mass of evidence presented to it. Looking solely at the facts in which all capable of investigation agree—facts which it is needless to recite, they having been so fully and ably stated in the works of Mr Lyell and Dr Buckland—we there see, and with no theoretic eye, the remains of animated things, more and more differing from existing races, as we descend in the series of strata. Not merely are the petrified bones preserved, displaying marks of the insertion of every muscle necessary for the movement of the living animl, but in some cases we discover even the secretions of their organs, prepared either for nourishment or for defence. Almost every stratum we pause to examine, affords indubitable evidence of having, at some former period, existed for ages at the bottom of some lake or estuary, some inland sea, or some extensive ocean teeming with animal existence, or of having been the surface of a country covered with vegetation, which perished and was renewed at distant and successive periods.

Those, however, who, without the knowledge which enables them to form an opinion on the subject, feel any latent wish that this evidence should be overthrown, would do well to remember that geology also furnishes strong evidence in favour of the much more direct statement of Moses, as to the recent creation of Man. And although we must ever feel a certain degree of caution in admitting negative evidence as conclusive; yet, in the present instance, the multitude of fossil bones which have been discovered, and which, when examined by persons *duly qualified* for the task, have been uniformly pronounced to be those of various tribes of animals, and not those of the human race, undoubtedly affords strong corroborative evidence in confirmation of the Mosaic account.

In truth, the mass of evidence which combines to prove the great antiquity of the Earth, is so irresistible, and so unshaken by any opposing facts, that none but those who are alike incapable of observing the facts and of appreciating the reasoning, can for a moment conceive the present state of its surface to have been the result of only six thousand years of existence.

What, then, have those accomplished who have restricted the Mosaic account of creation to that diminutive period, which is, as it were, but a span in the duration of the Earth's existence, and who have imprudently rejected the testimony of the senses, when opposed to their philological criticisms? Undoubtedly, if they have succeeded in convincing either themselves or others, that one side of the question must be given up as untenable, those who are so convinced are bound to reject that which rests on testimony, not that which is supported by still existing facts. The very argument which Protestants have opposed to the doctrine of transubstantiation * would, *if their view of the case were correct*, be equally irresistible against the book of Genesis.

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* The historian of the "Decline of the Roman Empire" carried the argument yet farther:—"I still remember (he remarks) my solitary transport at the discovery of a philosophical argument against transubstantiation; that the text of scripture, which seems to indicate the real presence, is attested only by a single sense—our sight; while the real presence itself is disproved by three of our senses—the sight, the touch, the taste."—*Gibbon's Memoirs of his Life*, vol. i. p. 58.

But let us consider what would be the conclusion of every reasonable being in a parallel case; let us imagine a manuscript written three thousand years ago, and professing to be a revelation from the Deity, in which it was stated, that the colour of the paper of the very book now in the reader's hand is *black*, and that the colour of the ink in the characters which he is now reading is *white*,—with that reasonable doubt of his own individual faculties, which would become the inquirer into the truth of a statement said to be derived from so high an origin, he would ask of all those around him, whether, to their senses, the paper appeared to be *black*, and the ink to be *white*. If he found the senses of other individuals agree with his own, then he would undoubtedly pronounce the alleged revelation a forgery, and those who propounded it to be either deceived or deceivers. He would rightly impute the attempted deceit to moral turpitude, to the gross ignorance, or to the interested motives of the supporters of it; and he certainly would not commit the impiety of supposing the Deity to have wrought a miraculous change upon the senses of our whole species, and to demand their belief in a fact directly opposed to those senses; thus throwing doubt upon every conclusion of reason which related to external objects, and, amongst others, upon the very evidence by which the authenticity of that questionable manuscript was itself supported, and even of its very existence when before their eyes.

Thus, then, had those who attempt to shew that the account of the creation in the book of Genesis is contradicted by the discoveries of modern science, succeeded, they would have destroyed the testimony of Moses, they would have uncanonized one portion of Scripture, and, by implication, have thrown doubt on the remainder. But minds which thus failed to trace out the necessary consequences of their own argument, were not likely to have laid very secure foundations for the basis on which it rested; and I shall presently prove that the contradiction they have imagined can have no real existence, that, whilst the testimony of Moses remains unimpeached, we may also be permitted to confide in the testimony of our senses.

Before entering on the main argument it may be remarked, that the plainest and most natural view of the language employed by the sacred historian of the Earth is, that his expressions ought to be received by us in the sense they were understood by the people to whom he addressed himself. If, when speaking of the creation, instead of using the terms light and water, he had spoken of the former as a wave, and of the latter as the union of two invisible airs, he would assuredly have been perfectly unintelligible to his countrymen. At the distance of above three thousand years his writings would just have begun to be comprehended, and possibly three thousand years hence, those views may be as inapplicable to the then existing state of human knowledge, as they would have been when the first chapter of Genesis was written.

Those, however, who attempt to disprove the facts presented by observation, by placing them in opposition to revelation, have mistaken the very groundwork of the question. The revelation of Moses itself rests, and must necessarily rest, on *testimony*. Moses, the author of the oldest of the sacred books, lived about 1500 years before the Christian era, or about 3300 years ago. The oldest manuscripts of the Pentateuch at present known, appear to have been written about 900 years ago.* These were copied from others of older date, and those again might probably, if their history were known, be traced up through a few transcripts to the original author; but no part of this is revelation; it is testimony. Although the matter which the book contains was revealed to Moses, the fact, that what we now receive as revelation is the same with that originally communicated, is entirely dependent on testimony. Admitting, however, the full weight of that evidence, corroborated as it is by the Samaritan version; nay, even supposing that we now possessed the identical autograph of the book of Genesis by the hand of its author, a most important question remains,—what means do we possess of translating it?

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* Mr Horne, in the *Introduction to the Critical Study of the Holy Scriptures*, states, that the total number of Hebrew MSS. collated by Dr Kennicott for his critical edition of the Hebrew Bible, was about 630. In that work Mr Horne gives an account of ten of the most ancient of these MSS., three of which contain the first chapter of Genesis, viz :—

No. 4. Codex Caesanae, in the Malatesta Library at Bologna, written about the end of the eleventh century.

No. 6. Codex Mediolanensis, written towards the close of the twelfth century. "The beginning of the book of Genesis, and the end of Leviticus and Deuteronomy, have been written by a later hand."

No. 8. Codex Parisiensis, 27, about the commencement of the twelfth century.

No. 10. Codex Parisiensis, 24, written at the beginning of the twelfth century.

In the same work is an account of six of the most ancient of the 479 collated by M. De Rossi. Two of these contain the first chapter of Genesis, and the date of both is about the end of the eleventh or beginning of the twelfth century. Of the manuscripts of the Samaritan versions of the Pentateuch, cited in the same work,—one, the Codex 197, in the Ambrosian Library at Milan, Dr Kennicott thinks, is certainly not later than the tenth century.

In similar cases we avail ourselves of the works of the immediate predecessors and of the contemporaries of the writer; but here we are acquainted with no work of any predecessor,—of no writing of any contemporary; and we do not possess the works of any writers in the same language, even during several succeeding centuries, if we except some few of the sacred books. How, then, is it possible to satisfy our minds of the minute shades of meaning of words, perhaps employed popularly; or, if they were employed in a stricter and more philosophical sense, where are the contemporary philosophical writings from which their accurate interpretation may be gained?

The extreme difficulty of such an inquiry will be made apparent by imagining a parallel case. Let us suppose all writings in the English, and, indeed, in all other languages previous to the time of Shakspeare, to have been destroyed,—let us imagine one manuscript of his plays to remain, but not a vestige of the works of any of his contemporaries; and, further, suppose the whole of the succeeding works of English literature to be annihilated nearly up to the present time. Under such circumstances, what would be our knowledge of Shakspeare? We should undoubtedly understand the general tenor and the plots of his plays. We should *read* the language of all his characters; and, viewing it generally, we might even be said to understand it. But how many words connected with the customs, habits, and manners of the time must, under such circumstances, necessarily remain unknown to us! Still further, if any question arose, requiring for its solution a knowledge of the minuter shades of meaning of words now long obsolete, or of terms supposed to be used in a strict or philosophical sense, how completely unsatisfactory must our conclusions remain? Such I conceive to be the view which common sense bids us take of the interpretation of the book of Genesis. The language of the Hebrews, in times long subsequent to the date of that book, may not have so far changed as to prevent us from rightly understanding, generally, the history it narrates; but there appears to be no reasonable ground for venturing to pronounce with confidence on the minute shades of meaning of allied words, and, on such foundations, to support an argument opposed to the evidence of our senses.

I should have hesitated in offering these remarks respecting the right interpretation of the Mosaic account of the creation, had the argument depended on any acquaintance with the language in which the Sacred Volume is written, or on any refinements of criticism, had I possessed that knowledge; but, in estimating its validity, or in supplying a more cogent argument, I entreat the reader to consider well the difficulties which it is necessary to meet.

1*st*, The Church of England, if we may judge by the writings of those placed in authority, has hitherto considered it to have been expressly stated in the book of Genesis, that the Earth was created about six thousand years ago.

2dly, Those observers and philosophers who have spent their lives in the study of geology, have arrived at the conclusion, that there exists irresistible evidence that the date of the Earth's first formation is far anterior to the epoch supposed to be assigned to it by Moses; and it is now admitted by all competent persons, that the formation, even of those strata which are nearest the surface, must have occupied vast periods,—probably millions of years,—in arriving at their present state.

3dly, Many of the most distinguished members of the Church of England now distinctly and formally admit the fact of such a lengthened existence of the Earth which we inhabit; for it is so stated in the eighth *Bridgewater Treatise*, a work written by the Professor of Geology in the University of Oxford—himself holding an office of dignity in that church, and expressly appointed to write upon that subject by the Archbishop of Canterbury and the Bishop of London.

4thly, The Professor of Hebrew at the same university has proposed a new interpretation of those passages of the book of Genesis which were hitherto supposed to be adverse to the now admitted facts.*

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* I have much satisfaction, says Dr Buckland, in subjoining the following note by my friend, E. B. Pusey, the Regius Professor of Hebrew in Oxford, as it enables me to advance the very important sanction of Hebrew criticism in support of the interpretations, by which we may reconcile the apparent difficulties arising from geological phenomena, with the literal interpretation of the first chapter of Genesis:—

"Two opposite errors have, I think, been committed by critics, with regard to the meaning of the word *bara*, created; the one, by those who asserted that it *must* in itself signify 'created out of nothing;' the other, by those who endeavoured, by aid of etymology, to shew that it *must* in itself signify 'formation out of existing matter.' In fact, neither is the case; nor am I aware of any language in which there is a word signifying *necessarily* 'created out of nothing;' as, of course, on the other hand, no word when used of the agency of God would, *in itself*, imply the previous existence of matter. Thus the English word *create*, by which *bara* is translated, expresses that the thing created received its existence from God, without in itself conveying whether God called that thing into existence *out of nothing*, or no; for our very addition of the words '*out of nothing*' shews that the word creation has not, in itself, that force; nor indeed, when we speak of ourselves as creatures of God's hand, do we at all mean that we were *physically* formed out of nothing. In like manner, whether *bara* should be paraphrased by 'created out of nothing' (as far as we can comprehend these words), or, 'gave a new and distinct state of existence to a substance already existing,' must depend upon the context, the circumstances, or what God has elsewhere revealed, not upon the mere force of the word. This is plain from its use in Gen. i. 27, of the creation of man, who, as we are instructed, chap. ii. 7, was formed out of previously existing matter, the 'dust of the ground.' The word *bara* is indeed so far stronger than *asah*, 'made,' in that *bara* can only be used with reference to God, whereas *asah* may be applied to man. The difference is exactly that which exists in English between the words by which they are rendered 'created,' and 'made.' But this seems to me to belong rather to our mode of conception than to the subject itself; for making, when spoken of with reference to God, is equivalent to creating. The words, accordingly, bara, *created*—asah, *made*—yatsar, *formed*, are used repeatedly by Isaiah, and are also employed by Amos, as equivalent to each other. *Bara* and *asah* express alike a formation of something new (de novo), something whose existence in this new state originated in and depends entirely upon the will of its creator or maker. Thus God speaks of himself as the creator, '*boree*,' of the Jewish people, *e. g.* Isaiah xliii. 1–15; and a new event is spoken of under the same term as 'a creation,' Numb. xvi. 30. English version, 'If the Lord make a new thing;' in the margin, Heb. 'create a creature.' Again, the Psalmist uses the same word, Ps. civ. 30, when describing the renovation of the face of

Such being the present state of the case, it surely becomes a duty to require a very high degree of evidence before we again

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the earth through successive generations of living creatures, 'Thou sendest forth thy spirit, they are *created;* and thou renewest the face of the earth.' The question is popularly treated by Beausobre, Hist. de Manichesime, tom. ii. lib. 5, c. 4; or in a better spirit, by Petavius, Dogm. Theol. tom. iii.; de Opificio sex Dierum, lib. i. c. i. § 8.

"After having continually re-read and studied this account, I can come to no other result than that the words 'created' and 'made' are synonymous (although the former is to us the stronger of the two), and that, because they are so constantly interchanged; as, Gen. i. ver. 21, 'God *created* great whales;' ver. 25, 'God *made* the beast of the earth;' ver. 26, 'Let us *make* man; ver. 27, 'So God *created* man.' At the same time it is very probable that bara, '*created*,' as being the stronger word, was selected to describe the first production of the heaven and the earth.

"The point, however, upon which the interpretation of the first chapter of Genesis appears to me *really* to turn, is, whether the two first verses are merely a summary statement of what is related in detail in the rest of the chapter, and a sort of introduction to it, or whether they contain an account of an act of creation. And this last seems to me to be their true interpretation, first, because there is no other account of the creation of the earth; secondly, the second verse describes the condition of the earth when so created, and thus prepares for the account of the work of the six days; but if they speak of any creation, it appears to me that this creation 'in the beginning' was previous to the six days, because, as you will observe, the creation of each day is preceded by the declaration that God said, or willed, that such things should be ('and God said'), and therefore the very form of the narrative seems to imply that the creation of the first day began when these words are first used, *i. e.* with the creation of light in verse 3. The time, then, of the creation in verse I. appears to me not to be defined: we are told only what alone we are concerned with, that all things were made by God. Nor is this any new opinion. Many of the fathers (they are quoted by Petavius, l. cc. II. § i–viii.) supposed the two first verses of Genesis to contain an account of a distinct and prior act of creation; some, as Augustine, Theodoret, and others, that of the creation of matter; others that of the elements; others again (and they the most numerous) imagine that, not these visible heavens, but what they think to be called elsewhere 'the highest heavens,' the 'heaven of heavens,' are here spoken of, our visible heavens being related to have been created on the second day. Petavius himself regards the light as the only act of creation of the first day (c. vii. de opere primæ diei, *i. e.* luce), considering the two first verses as a summary of the account of creation which was about to follow, and a general declaration that all things were made by God.

"Episcopius, again, and others, thought the creation and the fall of the bad angels took place in the interval here spoken of: and, misplaced as such speculations are, still they seem to shew that it is natural to suppose that a consiclaim authority for the opinion, that the book of Genesis contains such a precise account of the work of creation, that we may venture to appeal to it as a refutation of observed facts. The history of the past errors of our Parent Church supplies us with a lesson of caution which ought not to be lost by its reformed successors. The fact, that the venerable Galileo was compelled publicly to deny, on bended knee, a truth of which he had the most convincing demonstration, remains as a beacon to all after time, and ought not to be without its influence on the inquiring minds of the present day.

If the explanation offered by the Professor of Hebrew be admitted, those who adhere to it must still have some misgivings as to the effect of new discoveries in nature causing continual occasion for amended translations of various texts; whereas, should the view which has been advocated in this chapter be found correct, instead of fearing that the future progress of science may raise additional difficulties in the way of revealed religion, we are at once relieved from all doubt on that subject.—*Babbage's Bridgewater Treatise*, p. 63.

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derable interval may have taken place between the creation related in the first verse of Genesis, and that of which an account is given in the third and following verses. Accordingly, in some old editions of the English Bible, where there is no division into verses, you actually find a break at the end of what is now the second verse; and in Luther's Bible (Wittenberg, 1557), you have in addition the figure 1. placed against the third verse, as being the beginning of the creation of the first day.

" This, then, is just the sort of confirmation which one wished for, because, though one would shrink from the impiety of bending the language of God's Book to any other than its obvious meaning, we cannot help fearing lest we might be unconsciously influenced by the floating opinions of our own day, and therefore turn the more anxiously to those who explained Holy Scripture before these theories existed. You must allow me to add, that I would not define further. We know nothing of creation, nothing of ultimate *causes*, nothing of space, except what is bounded by actual existing bodies, nothing of time, but what is limited by the revolution of those bodies. I should be very sorry to appear to dogmatize upon that, of which it requires very little reflection or reverence to confess that we are necessarily ignorant. 'Hardly do we guess aright of things that are upon the earth, and with labour do we find the things that are before us; but the things that are in heaven, who hath searched out?' Wisdom ix. 16. E. B. PUSEY."—*Buckland's Bridgewater Treatise*, pp. 22-5.

Notice of the Result of an Experimental Observation made regarding Equivocal Generation. By F. Schulze, Berlin.

Since the question respecting *generatio æquivoca* has attracted the attention of naturalists, the development of living organisms has never been observed in vessels from which all air had been expelled by boiling, and which had been hermetically sealed. The access of air has been regarded as a necessary condition for the primary formation of infusoria from decomposing organic matter, so that the mere circumstance of covering an infusion with a stratum of oil, removed that condition. But the question still remained undecided, If the access of atmospheric air, light, and heat to *infundirten* substances, included of itself all the conditions for the primary formation of animal or of vegetable organisms ? And in this point of view new direct experiments were considered to be very desirable. The difficulty to be overcome consisted in the necessity of being assured, first, that at the beginning of the experiment there was no animal or germ capable of development in the infusion; and secondly, that the air admitted contained nothing of the kind. For this purpose I constructed the apparatus represented in Plate I. fig. 2.

I filled a glass flask half full of distilled water, in which I mixed various animal and vegetable substances; I then closed it with a good cork, through which I passed two glass tubes bent at right angles, the whole being air-tight. It was next placed in a sand bath, and heated until the water boiled violently, and thus all parts had reached a temperature of 212° F. While the watery vapour was escaping by the glass tubes, I fastened at each end an apparatus which chemists employ for collecting carbonic acid; that to the left was filled with concentrated sulphuric acid, and the other with a solution of potash. By means of the boiling heat every thing living, and all germs in the flask or in the tubes, were destroyed, and all access was cut off by the sulphuric acid on the one side, and by the potash on the other. I placed this easily moved apparatus before my window, where it was exposed to the action of light, and also, as I performed my experiments during the summer, to that of heat. At the same time I placed near it an open vessel, with the same substances that had been introduced into the flask, and also after having subjected them to a boiling temperature. In order now to renew constantly the air within the flask, I sucked with my mouth, several times a-day, the open end of the apparatus filled with solution of potash ; by which process the air entered my mouth from the flask through the caustic liquid, and the atmospheric air from without entered the flask through the sulphuric acid. The air was of course not at all altered in its composition by passing through the sulphuric acid in the flask, but if sufficient time was allowed for the passage, all the portions of living matter, or of matter capable of becoming animated, were taken up by the sulphuric acid and destroyed. From the 28th May till the beginning of August, I continued uninterruptedly the renewal of the air in the flask, without being able, by the aid of the microscope, to perceive any living animal or vegetable substance, although during the whole of the time I made my observations almost daily on the edge of the liquid ; and when at last I separated the different parts of the apparatus, I could not find in the whole liquid the slightest trace of infusoria, of confervæ, or of mould. But all three presented themselves in great abundance a few days after I had left the flask standing open. The vessel which I placed near the apparatus contained on the following day vibriones and monades, to which were soon added larger polygastric infusoria, and afterwards rotatoriæ.

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1. *On the Elevation of Beaches by Tides.* 2. *On Ripple-Mark.*

1. *Elevation of Beaches by Tides.*

If the earth were a spheroid of revolution, covered by one uniform ocean, two great tidal waves would follow each other round the globe at a distance of twelve hours.

Suppose several high narrow stripes of land were now to encircle the globe, passing through the opposite poles, and dividing the earth's surface into several great unequal oceans, a separate tide would be raised in each. When the tidal wave had reached the farthest shore of one of them, conceive the causes that proce it to cease : then the wave thus raised would recede to the opposite shore, and continue to oscillate until destroyed by the friction of its bed. But if, instead of ceasing to act, the causes which produce the tide were to reappear at the opposite shore of the ocean, at the very moment when the reflected tide had returned to the place of its origin; then the second tide would act in augmentation of the first, and, if this continued, tides of great height might be produced for ages. The result might be, that the narrow ridge dividing the adjacent oceans would be broken through, and the tidal wave traverse a broader tract than in the former ocean. Let us imagine the new ocean to be just so much broader than the old, that the reflected tide would return to the origin of the tidal movement half a tide later than before; then, instead of two superimposed tides, we should have a tide arising from the subtraction of one from the other. The alterations of the height of the tides on shores so circumstanced, might be very small, and this might again continue for ages; thus, causing beaches to be raised at very different elevations, without any real alteration in the level either of the sea or land.

If we consider the superposition of derivative tides, similar effects might be found to result; and it deserves inquiry, whether it may not be possible to account for some remarkable and well-attested phenomena by such means.

The gradual elevation, during the past century, of one portion of the Swedish coast above the Baltic, is a recognised fact, and has lately been verified by Mr Lyell.* It is not probable, from the form and position of that sea, that two tides should reach it distant by exactly half the interval of a tide, and thus produce a very small tide; nor is it likely that, by the gradual but slow erosion of the longer channel, one tide should almost imperceptibly advance upon the other; but it becomes an interesting question to examine whether, in other places, under such peculiar circumstances, it might not be possible that a series of observations of the heights of tides at two distant periods might give a different position for the mean level of the sea at places so situated.

If we conceive two tides to meet at any point, one of which is twelve hours later than the other, the elevation of the waters will arise from the joint influence of both. Let us suppose, that, from the abrasion of the channel, the latter tide arrives each time one-hundredth of a second earlier than before. After about 3150 years, the high water of the earlier tide will coincide in point of time with the low water of the latter tide; and the difference of height between high and low water will be equal to the difference of the height of the two tides, instead of to their sum, as it was at the first epoch.

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* See Phil. Trans. 1836.

If, in such circumstances, the two tides were nearly equal in magnitude, it might happen that on a coast so circumstanced, there would, at one time, be scarcely any perceptible tide; and yet, 3000 years after, the tide might rise thirty or forty feet, or even higher; and this would happen without any change of relative height in the land and water during the intervening time. Possibly this view of the effects which may arise, either from the wearing down of channels, or the filling up of seas through which tides pass, may be applied to explain some of the phenomena of raised beaches, which are of frequent occurrence.

Natural philosophers are at present not quite agreed upon the mode of determining the mean level of the ocean. Whether it is to be deduced from the averages between the highest and lowest spring tide, or from the averages of all the intermediate ones, or from the means of the instantaneous heights of the tide at all intervening periods, or by whatever other process, its true level is yet to be ascertained. It may, perhaps, be useful to suggest that, besides the actual level of the sea at any particular place, it would be also desirable to ascertain whether the time of high water at given epochs is not itself a changeable quantity. These reflections, however, are only thrown out with the view of exciting discussion on a subject involved at present in great mathematical difficulties, and possessing, at the same time, the highest practical importance.*—*Babbage's Bridgewater Treatise.*

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* The great temporary floods which have occasioned the deposition of the remains of the ocean, considerably above the level of the sea, such as the great flood described by Boethius, which, he says, "happened in the Ides of October, in the year of Redemption 1097, carrying destruction into the country, overwhelming villages, castles, towns, and extensive woods," may explain some of these raised beaches.—Edit.

2. *On Ripple-Marks.*

The small waves raised on the surface of the water, by the passage of a slight breeze, are called ripple; and a series of marks, very similar in appearance, which are sometimes seen at low water, on the flat part of a sea-beach formed of fine sand, are called ripple-marks. Such marks occur in various strata; and are regarded as evidence of their having been formed beneath the sea. Similar appearances occur when a strong wind drives over the face of a sandy plain.

It appears that two fluids of different specific gravity, the lighter passing over the surface of the former, always concur in the formation of ripple. It seems also, that the lines of ripple-mark are at right angles to the direction of the current which forms them.

If a fluid-like air, pass over the surface of perfectly quiescent water, in a plain absolutely parallel, it will have no effect; but if it impinge on the surface of that water with the slightest inclination, it will raise a small wave, which will be propagated by undulations to great distances. If the direction of the wind is very nearly parallel to the surface of the water, this first wave, being raised above the general surface, will protect that part of the water immediately beyond it from the full effect of the wind, which will therefore again impinge upon the water at a little distance; and, this concurring with the undulation, will tend to produce another small wave, and thus again, new waves will be produced. But the under surface of the air itself will also assume the form of waves; and so, on the slightest deviation at any one point from absolute parallelism in the two fluids, their whole surfaces will become covered with ripples.

If one of the fluids be water, and the lower fluid be fine sand, partially supported in water, these marks do not disappear when the cause ceases to act, as they do when formed by air on the surface of water. These are the marks we observe when the tide has receded from a flat sandy shore.

If, after the formation of ripple-marks at the bottom of a shallow sea, some adjacent river, or some current, deposit upon them the mud which it holds in suspension, then the former marks will be preserved, and new ripple-marks may appear above them. Such is the origin of those marks we observe in various sandstones, from the most recent down to those of the coal-measures.

Dr Fitton informs me, that he found the sand-hills on the south of Etaples (in France), consisting of ripple-marks on a large scale. They are crescent-shaped hillocks, many of which are more than a hundred feet high. The height is greatest in the middle of the crescents, declining towards the points; and the slope on the inner side of the crescent, which is remote from the prevailing direction of the winds, is much more rapid than that on which it strikes.

Mr Lyell has observed and described this mode of formation of ripple on the dunes of sand near Calais; remarking, that in that case there is an actual lateral transfer—the grains of sand being carried by the wind up the less inclined slope of the ripple, and falling over the steep scarp. I have observed the same fact at Swansea.

A similar explanation seems to present itself as the origin of that form of clouds familiarly known as "a mackarel sky"—a wave-like appearance, which probably arises from the passage of a current of air above or below a thin stratum of clouds. The air being of nearly the same specific gravity as that of the cloud it acts upon, would produce ripples of larger size than would otherwise occur.

The surface of the sun presents to very good telescopes a certain mottled appearance, which is not exactly ripple, and which it is difficult to convey by description. It may, however, be suggested, that wherever such appearances occur, whether in planetary or in stellar bodies, or in the minuter precincts of the dye-house and the engine-boiler, they indicate the fitness of an inquiry, whether there are not two currents of fluid or semi-fluid matter, one moving with a different velocity over the other, the direction of the motion being at right angles to the lines of waves.—*Babbage's Bridgewater Treatise*, p. 222.

On Rippoldsau and its Mineral Waters. By the Rev. Edward Stanley. Communicated by the Author.

Below I annex an analysis of the mineral waters of Rippoldsau, one of the many, but, to the generality of travellers, least known, of the attractive Brunens, so abundantly scattered over the parts of Germany included between the upper parts of the Rhine and the Neckar. It is situated nearly in the centre of the Black Forest in a narrow valley (at the foot of the Kniebis, one of the highest mountains of that district) through which the River Wolf winds its way, till it falls in with Kinzig, which joins the Rhine a short distance below Strasburg. From Baden, through the romantic vale of the Murg, it may be reached in rather less than nine hours, stoppages not included, by an excellent road, through Gernsbach, Forbach, Schwartzenburg, and Freudenstadt, where the road turns abruptly to the westward, and gradually ascends the Kinebis, near the highest elevation of which, it again turns abruptly to the south, and by a very steep descent, plunges into the deep valley, in the midst of which, as if fallen from the clouds, stands Rippoldsau, a small village, or rather collection of accommodations for travellers, where, to their surprise, in the midst of this apparent solitude, they will find themselves seated, in one of the most singular and beautiful dining-rooms, at a table d'hote, with from 150 to upwards of 200 guests to bear them company. The property belonged originally to the grand Duchy of Baden, but was purchased about ten years ago of Prince Furstenberg, by the present proprietor M. Goeringer, who has speculated largely in improvements and buildings, which promise to yield an ample remuneration, there being few similar places, which, in point of scenery, mineralogy, and mineral waters can rival this secluded spot. Two roads will conduct the traveller to Strasburg, an upper through Eppenau and Oberkirk, and a lower by Hausach and Offenburg, the latter occupying about eight and a half hours, stoppages not included.

The analysis from each of the three springs, named Joseph, Leopold, and Wenzel, is from a measure of sixteen French ounces.

	Spring of		
	Joseph,	Leopold,	Wenzel.
	grains.	*grains.*	*grains.*
Carbonic of Lime, ...	9.48	6.15	5.30
Sub-carbonate of Iron, ...	.76	.62	.43
Sub-carbonate of Manganese, ...	.57	.40	.32
Carbonate of Magnesia, ...	.16	.20	.09
Sulphate of Soda crystallized, ...	15.60	12.20	8.87
Sulphate of Lime, ...	.43	.30	.26
Phosphate of Soda, ...	.24	...	.14
Phosphate of Alumine and Magnesia, . . .	.18	...	.21
Silicate of Alumine, ...	1.09	.33	.67
Muriate of Soda, ...	.12	.46	.08
Do. Potash, slight traces of, in the springs of Joseph and Wenzel only, ...	...	...	...
Do. do. Magnesia, ...	.24	.34	.14
Bituminous extracts and traces of fluate of Lime,	.12	...	.09
Sulphate of Potash, ...	...	.51	...
Bituminated Hydro-sulphuret, ...	...	.20	...
	29.04	24.37	[illegible] 3
Carbonic acid gas in French cubic inches, .	32.40	28.50	23.60
Temperature in degrees of Fahrenheit, .	+ 50°	+ 52°	+ 50°

Remarks on the Origin of Amber. By H. R. Göppert[*].

While, during the month of April of this year, I was engaged in the examination of the deposit of brown coal at Muskau, I discovered, besides a rhizomorpha and a lichen allied to the Pyrenula nitida (the only representatives of this family at present known in the flora of the ancient world), a large quantity of amber, which occurs in fossil wood apparently coniferous, partly in large disseminated portions, and partly in the resin vessels themselves. A fir cone, found there by Mr Kehlosen, director of the alum manufactory, approaches most nearly the cone of the Pinus sylvestris, but differs exceedingly from some others which were obtained at Salyhausen in the Wetterau, and which have been kindly placed at my disposal by Mr Keferstein. These belong evidently to the genus Abies, and still contain between and on the scales a large quantity of amber, and may therefore with much greater reason be regarded as the cones of the amber-tree, than the cones found included in amber. Specimens of the latter description are extremely rare, but Dr Behrendt of Dantzic, and Professor Reich of Berlin, each possess one, and both are very nearly allied to the genus Larix, as was formerly remarked of the last specimen by Professor Link, (*Handbuch der physikalischen Erdbeschsreibung*, vol. ii. part 1, p. 333) : both belong to the same species, and only differ in size. Besides alluding to these vegetable productions yielding amber, all of which have been found in the brown coal formation, I have to mention an observation which is probably new, and which has been communicated to me by Dr Schneider of Bunzlaw, viz. that amber occurs in coniferous plants associated with ferns in the coal of Wenig-Rackvity, a deposit that has been referred by Raumer to the quader-sandstone. Since then it appears that we already know four different species of tree which afford amber, (and the number would doubtless be increased by attentive investigation), the probability seems to me to be rendered still stronger, *that amber is nothing else than an indurated resin derived from various trees of the family of the Coniferæ ; which resin is found in a like condition in all zones, because its usual original depositaries, viz. beds of brown coal, have been formed almost every where under similar circumstances.*

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[*] From Poggendorff's Analen der Physik und Chemie, 1836.

On the Use of Steam, in the Economising of Fuel. By ANDREW FYFE, M. D., F. R. S. E., M. S. A.*

IT has been long known that when steam is passed over charcoal, at a red heat, excluded from air, a gaseous inflammable matter is evolved, which has by some been considered as a species of *hydro-carbon*. I have been induced to investigate the nature of this action, partly with the view of ascertaining the composition of the gaseous fluid, and partly also to ascertain how the substances, commonly used as fuel, are acted on by steam, when they are undergoing combustion.

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* Read before the Society of Arts.

The action of water on incandescent charcoal was, I believe, first noticed by Priestley, and the examination of the gaseous product was afterwards undertaken by Cruickshank, and also by Berthollet; the former of whom was inclined to consider it a compound of *carbon*, *hydrogen*, and *water*; 100 cubic inches (weighing 14.5 gr.) containing, according to him, 4 gr. carbon, 1.3 of hydrogen, and 9 of water, while Berthollet concluded, from his experiments, that it is composed of 5 by weight of carbon, and 4 of hydrogen.

These discordant results shew, either that the gas generated varies in its composition, according to the method followed in its preparation, or, that there were sources of fallacy in the experiments, owing to the imperfect means of analysis then in use. In preparing the gas for my experiments, I had recourse to the usual method, viz. passing steam through a porcelain tube, stuffed sometimes with charcoal, sometimes with coke, and heated to redness in a furnace; always taking care to have them heated for some time before propelling the steam over them. The gas was collected generally over a water trough. It was at first slightly opaque, but soon became transparent. When kept over lime-water there was a diminution, and the water became turbid, shewing the presence of carbonic acid. In one instance in which the gas was collected over mercury, the diminution amounted to 18 per cent. After removing the carbonic acid the sp. gr. was found to be in general about 470; occasionally, however, it was a little higher, but never beyond 480. When heated in contact with air it burns with a blue flame, and carbonic acid and water are formed, shewing the presence of carbon and hydrogen. In its combustion it differs from light hydro-carbon, not only in the appearance of its flame, but also in its combustibility with different proportions of air and of oxygen. The light hydro-carbon cannot be exploded by means of electricity, with more than two and a fourth of oxygen, whereas this gas may be exploded with four times its bulk of it. The action with spongy platinum is peculiar. When propelled against the sponge, there is no action at a natural temperature; but when the sponge is heated it inflames the gas. A mixture of equal parts of the gas and oxygen is not acted on by the sponge unless previously heated. Even when kept in contact with the sponge before a fire, there is no immediate action, but in the course of a few hours there is a very slight diminution in volume. Though the sponge does not cause any immediate action, yet an action does occur when they are kept together for some time. In one instance the mixture exploded on the fourth day; and in another, in which case it was put over the sponge on a Friday, it exploded at some time during the Sunday.

These experiments also shew that in its action with spongy platinum, the gas differs from light hydro-carbon. According to Henry a mixture of it and oxygen is not acted on by the sponge, even when heated to 555°.

When the gas is exposed to the action of chlorine over water, there is no action in the dark; but on exposure to sunshine, muriatic and carbonic acid are formed. The amount of condensation I have found to vary, but in no instance did the gas consume nearly as much chlorine as light hydro-carbon does, which requires no less than four volumes for complete condensation; and yields its own volume of carbonic acid and eight of muriatic, four of which are formed by the hydrogen of the water, the other four by that in the gas. The gas evolved by the action of steam on coke never gave so much as half its volume of carbonic acid, and not much more than its own bulk of muriatic acid, shewing evidently that it contained a very small quantity of hydrogen, in comparison with that in hydro-carbon.

From the experiments which have now been related, there can, I think, be no doubt, that if any hydro-carbon exists in the gas, it must be in very trifling quantity; but as carbonic acid is formed, both during its combustion, and also by its action with chlorine, carbon must exist in it, and if so, supposing it not in union with hydrogen, the only other state of combination in which it can be, must be with oxygen, in the form of *carbonic oxide.*

That the gaseous fluid is merely a mixture of carbonic oxide and hydrogen, may, I think, be inferred from what happens during its formation, taken in conjunction with the facts above mentioned. In one instance the gaseous product evolved by passing steam over charcoal, previously well heated, was collected over mercury, and treated with potassa, by which it lost 18 per cent., of course, of carbonic acid. Now, if the whole of the oxygen of the steam had united with carbon, to form carbonic acid, and supposing the hydrogen in the state of hydro-carbon, then the carbonic acid ought to have amounted to 50 per cent.; that is, the acid and hydro-carbon ought to have been in equal volumes; because water consists of 1 of oxygen, and 2 of hydrogen, and 1 of oxygen yields 1 of carbonic acid, and 2 of hydrogen also 1 of hydro-carbon. Again, supposing the hydrogen uncombined, the carbonic acid ought to have amounted to 33.3 per cent., or one half of the volume of the hydrogen, but it was only 18 per cent. Now, if we deduct the oxygen in this from the total amount of oxygen in the steam, there remains 15.3, which, if we suppose it in the state of carbonic oxide, would yield 30.6 of that gas; so that the oxide and hydrogen should in the residual gas, after the abstraction of the carbonic acid, be in the ratio of 30.6 : 66.6, that is as 1 to 2.17, or say in round numbers as 1 to 2.

That this is the composition of the gas, is confirmed by explosion with oxygen in the Volta eudiometer. Though the results varied in different trials, it yielded generally about one-third of its volume of carbonic acid. Now, carbonic oxide when fired with oxygen yields its own volume of acid; so that supposing the gaseous fluid to contain one-third of its bulk of oxide, it ought to yield the same quantity of acid, which it generally did. Of course, in giving this as its composition, I do not mean to say that it invariably contains exactly these proportions. The difference in specific gravity, and the different results obtained by the action of chlorine, and by the Volta eudiometer, shew that the proportions vary, probably owing to the temperature, or the rapidity with which the steam is passed over the incandescent charcoal.

I may here mention that I have endeavoured to detect oxygen in the gas, by exposing it, after being freed of hygrometric moisture, to the action of potassium, which decomposes carbonic oxide; depriving it of its oxygen, and causing a deposit of carbon. Though potassium was kept in it for some days, it was not in the least affected; nor was there any diminution of the gas. I found, however, that when potassium was exposed to carbonic oxide, mixed with about twice its bulk of hydrogen, and both previously dried, it did not suffer any change, though they were kept for some days together, so that we are not, from the want of action with potassium, to infer that the gas evolved by steam acting on charcoal, does not contain oxygen.

The preceding experiments are, I think, sufficient to warrant the conclusion, that when steam is passed over incandescent carbon, the resulting gas is one containing *hydrogen*, *oxygen*, and *carbon ;* the two last in the state of carbonic oxide. Of course during its combustion it gives rise to the formation of carbonic acid and water.

This being the case, it occurred to me, that, by passing steam over charcoal or coke in a state of combustion, and to which *air is freely admitted*, the water might also be consumed, and give rise to the formation of inflammable gaseous matter, which, being likewise inflamed and consumed, might increase the heat during the combustion of the fuel ; that, in fact, the water, if I may be allowed the expression, might also be *burned*, or *used as fuel*, along with the other materials commonly employed for that purpose. In endeavouring to ascertain this, it was natural first to try whether, by the transmission of steam over burning coke, there was any increase of temperature ; and for this purpose a small furnace was used, having an opening at the side and near the bottom, into which was introduced an iron pipe connected with a boiler, by which steam was supplied. The furnace being kindled with *coke*, and brought into *good condition*, a vessel with water was placed over it, and the time required to cause the water to boil was noted, using the same vessel, and the water always at the same temperature (50° F.), in all the different trials. The following is the result of a few of numerous experiments. The vessel contained two pints of water.

Without Steam.	With Steam.
In 5 min. 130°; in 10, 180°; 15, 200°; 20, 210°.	In 5 min. 120°; 10, 185°; 13, 208° boiled briskly in 14 min.
In 5 min. 145°; boiled in 10½.	In 5 min. 150°; boiled in 10.
Again, do. in 10.	do. in 9.

In other trials only one pint of water was used.

In 3 min. 120°; 5, 160°; 7, 190°; 8½ boiled.	In 3 min. 140°; 5, 190°; 6½, boiled.
In 3 min. 140°; 5, 190°; 6¼ boiled.	In 3 min. 160°; in 5 boiled.

In numerous other experiments performed in a similar manner, the results were always the same, the water invariably boiling more rapidly when steam was transmitted through the furnace, so that there is evidently an increase of heat. The following experiments on the *quantity of water evaporated* in a given time, are also in *proof of this.* The same furnace and steam apparatus were used as in the preceding trials, but a smaller vessel was employed for the evaporation of the water. Furnace in good condition, containing coke. Half a pint of water in the pan.

Without Steam.	With Steam.
In 2½ min. boiled; in 8 min. on furnace, lost 4 oz.	In 2¼ boiled; in 8 min. lost 5 oz.

With one pint of water—

In 7 min. boiled; in 15 lost 4¼ oz.	In 5 boiled; in 15 lost 7 oz.
In 4 min. boiled; in 12 lost 6¾.	In 3¾ boiled; in 12 lost 7¼.

In all of these experiments I found, that, when the steam was used, it required the *air to be freely admitted* to the inflammable matter. Indeed, when this was not done, instead of there being an increase, there was less heat; the water in the evaporating pan not being made to boil so quickly when steam was passed through the fuel as when it was omitted.

Though these experiments prove that there is an increase of heat during the combustion of fuel when steam is passed through it, it became a question whether or not the heat gained is merely that which is carried in by the steam, and which it has acquired from another source. To put this to the test of experiment, it was necessary to compare the quantity of steam transmitted through the fuel with that given off from the evaporating vessel. In one experiment, in which the ebullition was kept up for ten minutes, the difference in the quantity of water evaporated with steam transmitted and without it, was two and three-fourths of an ounce, whereas the quantity transmitted amounted to only half an ounce, or about one-fifth. In another trial the difference amounted to four times the quantity of steam thrown in, and in others it was occasionally a little more, sometimes a little less, according to the rapidity with which the water was boiled in the boiler. On an average I would say, that, for each ounce of steam thrown into the furnace, there were *four* ounces additional evaporated over and above that evaporated without the transmission of steam, provided the steam was thrown in *cautiously*, because, when forced in in too great quantity, as before stated, there is no increase, perhaps owing to the whole of it not being decomposed and consumed.

There can be no doubt, I think, with regard to the cause of the increase of heat in these experiments. It must proceed from the *gaseous* matter generated by the action of the water on the fuel, being inflamed as it proceeds up through the furnace; and hence the necessity of a greater supply of air than when fuel alone is used. That there really is the combustion of gaseous matter when steam is transmitted, is proved by the change on the appearance of the fuel, for no sooner is the steam admitted than flame is seen to issue profusely from it, and which ceases the moment that we discontinue its transmission. This experiment is well illustrated by using a small furnace, open above, and having the fuel burning briskly, and without flame, in a darkened room. On transmitting the steam from below, the flame will be observed several inches above the fuel.

Though there is an increase of heat, and, of course, may be a great saving of time in many processes, by transmitting steam through fuel, another very important point remained to be determined, viz. whether this is not gained at an additional expenditure of fuel in the furnace, and which at first sight one would naturally suppose would occur, owing to the action of the water on it.

In conducting this part of the investigation, I have had to contend with difficulties, which almost compelled me to relinquish the farther investigation of it; for though, from some experiments which were performed, I had every reason for inducing me to expect a favourable result, yet, in many, the results were not only not satisfactory, but were really such, as, were there no sources of error, ought to have warranted the conclusion that there is an additional consumpt of fuel, and, consequently, no saving by the process recommended.

In the trials which I first made, I found that when the fuel and steam passed through it, it was not more rapidly consumed than when it was burned without it. Thus, in one case, in which 32 parts of coke were put into the furnace, and the combustion kept up for an hour and a half, at the expiry of this, 25¼ parts were consumed; and when the same quantity was used during the same time, *along with steam*, there were only 23¾ consumed. In other trials similar results were obtained; the quantity of coke consumed, when steam was transmitted through it, sometimes being less, at other times very nearly the same. In one instance only it was greater, and, in that case, the *additional* consumpt amounted to only $\frac{1}{20}$th of the total consumpt.

In these trials, the proportion of fuel consumed was in the ratio of

With Steam.	Without it.	Excess.
840	970	130
4840	5190	350
1330	1370	40
1330	1570	240

Now, connecting this fact with the results of experiments on the quantity of water evaporated, in a given time, which, it has been shewn, is always greater when steam is used than when omitted, there was every reason for believing, that there was really something gained by the use of steam; but when I next attempted to compare the actual quantity of water evaporated, with the fuel used, I found in my first trials, that sometimes it was not greater, indeed, in many trials, it was less. This naturally led me to suspect some source of error; accordingly, in repeating the experiments over and over again, varying them in every way I could think of, I at last found, that, towards the end of the process, that is, when the fuel was nearly exhausted, and consequently the temperature was comparatively low, the steam seemed to escape decomposition. I was induced, therefore, to perform the experiments in a different way, and the results were then as I expected. The method followed, was to keep up the action, with and without steam, for a shorter period than I had done before, taking care to have the furnace all the time *in good condition*, instead of allowing nearly the whole of the fuel to be used, and of course having the heat at some time too low. In this method of conducting the process, I found, with very few exceptions, that there was rather less fuel consumed, when steam was transmitted through it than when omitted, while, at the same time, the quantity of water evaporated was increased.

It has been suggested to me that the increase of heat, by causing steam to pass through fuel, may be owing, in a great measure, to its acting merely as a *blast*, by carrying in a large supply of air along with it. That this is not the case, is proved, I think, by experiments which I have since performed, by introducing the steam in different ways, more particularly by uting the steam-pipe to the side of the furnace, to prevent e access of air along with it; in all of which cases, there was so an increase of heat. Besides, I have always found, that when the steam was introduced with *great rapidity*, the increase of heat was by no means so great as when thrown in *slowly*. Did the steam merely act as a blast, it ought to have been the reverse.

The experiments now detailed, are, to my mind, sufficient to prove, that, by the transmission of steam, there is a considerable increase of heat, without additional expenditure of fuel; that, in fact, the water, while passing, in the state of steam through fuel, not only acts as a sort of blast, but, at the same time, it self undergoes combustion, by the formation and consequent consumption of inflammable gaseous products.

It may, however, here be objected to the use of steam, that there must be a certain waste of fuel for the conversion of the water into vapour. This must certainly be the case in many instances; but, in most places where furnaces are in use, there is always the means of procuring steam, without any additional expenditure of fuel. Even allowing that there is not, yet the increase of heat, by a comparatively small quantity of steam, would, I think, more than compensate for any extra expenditure for converting the fluid into vapour. But why, it may be asked, not use the water in the fluid form? The answer to this is easily given. Not only must the fluid, the moment it touches the fuel, deprive that part of the fluid of caloric, and thus in a great measure diminish, if not entirely put a stop to, the combustion there; but also, by being imperfectly converted into vapour, it must also be imperfectly acted on by the fuel, and of course but partially undergo combustion. Hence, most likely the cause of failure, in those cases in which a mixture of tar and water has been allowed to fall, in a small stream, on fuel, as in the process lately recommended in some of the gas establishments. In this case, the object in using the water is altogether different from that which I propose. It is there used with the view of *yielding oxygen* to the inflammable matter of the tar; but, as I have already stated, it is not likely there should, in this way of using it, be any increase of heat; because, allowing that the oxygen of the water were to act on the inflammable matter, that oxygen must itself, in uniting with the carbon, and assuming the elastic form, acquire heat to retain it in that state, whereas, in using steam, the latent heat has been *already acquired*, and then, in undergoing decomposition, and giving rise to the formation of carbonic oxide and hydrogen, these, provided air is freely supplied, are consumed, and, during their union with the oxygen of the air, in their passage up through the furnace, will also give forth heat.

The only instance on record that I am acquainted with, in which steam was passed through fuel with any definite object in view, is that mentioned by Mr Mushet, of the Clyde Iron-works, in the 6th volume of *Tilloch's Magazine*. In alluding to the different means of procuring blasts for iron-furnaces, he states, among others, the *water-vault*, by which air is thrown into chests inverted in water, by means of pumping cylinders; and, in which case, the air is of course loaded with moisture. Though, by this means, a steady cool blast is procured, yet, according to Mr Mushet, there is one objection to its use, viz. the tendency which the air has to take up a considerable portion of moisture, and introduce it into the furnace, by which he seems to dread, if I may be allowed to judge from what occurs in another part of the paper, an intense temperature excited by the action of the moisture; for he there states, that when a loose blast is surcharged with moisture, the inflammation which takes place at the *tuyre is prodigious;* fine fire-clay will be fused, and blown to slug in a few minutes, and even the sides of the furnace are apt to give way. Though thus admitting the fact of the increase of heat, when air loaded with moisture is introduced into the furnace, yet he accounts for the inferior produce of cast-iron in *summer*, compared with that obtained in *winter*, by the state of the air, with regard to moisture; the quality of the air, according to him, *becoming contaminated for combustion, by holding in solution a much greater quantity of moisture.* At the same time, he states a fact, which I conceive corroborates what I have advanced with regard to the action of water on fuel in furnaces. The presence of moisture in the atmosphere, he says, will tend in a great measure to solve the curious phenomenon, of pig-iron taking up less carbon in summer than in winter, although reduced with a superior quantity of fuel. The air discharged most probably contains less oxygen, yet the metal is much less carbonated than at other times, and he conceives that this may be owing to part of the carbon being carried off by the hydrogen of the moisture. The inferior quality of pig-iron thus manufactured, owing perhaps to the abstraction of part of the carbon by the moisture, and also, as stated by Mr Mushet, to the inferior quantity of oxygen in a certain volume of the air, induced an individual to try the introduction of steam into the furnace, in the hope that, by supplying oxygen, the combustion would be more complete. The attempt proved a failure, owing to the cooling effect immediately where the steam was introduced, and the intense heat excited higher in the furnace, by which, in the course of time, the materials becoming fluid, the furnace was actually closed up. The object in view in using steam in this instance, is different from that which I have proposed. I conceive that it will, if properly introduced, increase the heat by the combustion of its hydrogen, whereas, in the case above mentioned, it was used under the impression that it would do so, by yielding oxygen to the fuel; and that this is what was expected, is evident from Mr Mushet's own words; for he says, that the decomposition of the water, by *furnishing a superior quantity of oxygen*, increases the effects of combustion, &c.

In the trial alluded to, Mr Mushet does not admit that there is any increase in the *total amount of heat* in the furnace. There is, he says, an increase *in the immediate vicinity of the chemical analysis, i. e.* of the decomposition of the water; yet, as the water had abstracted heat from the inferior strata, in its ascent it meets decomposition, and there yields up the abstracted heat, and also that contained in the oxygen of its decomposition, so that, in fact, it merely conveys the heat from one part of the furnace to the other, and there is, therefore, no actual increase in the amount of heat throughout the furnace. In this I do not agree with him. That there is really an increase in the actual amount of heat when steam is properly introduced, is, I think, satisfactorily established by the experiments I have mentioned.

Though the use of steam in iron furnaces may prove injurious, owing to the abstraction of part of the carbon, I consider the trial recorded by Mr Mushet important, because it corroborates what I have before mentioned, that, instead of steam being used with the view of supplying oxygen to the inflammable matter, air, in other words oxygen, must be freely admitted along with the steam, so as to keep up the combustion of the inflammable products, formed by the decomposition of the steam itself. Hence, in those experiments in which I introduced steam in too great quantity, while, at the same time, the air was not admitted in proportion, the heat, instead of being increased, was diminished.

I may here mention, that in some trials which I have made with high pressure-steam, which of course, when allowed to escape, rushed with great force upon the fuel, the effect was, to diminish the heat in the immediate vicinity of the point of escape, in fact, almost to extinguish the combustion at that particular part. I had no means of ascertaining at the time, whether, in its ascent through the furnace, it would undergo decomposition and combustion; most probably it would, provided the heat was sufficient, and there was also a proper supply of air.

The same high pressure-steam, I may also state, almost completely extinguished a chauffer containing coke; indeed I was prepared to expect this, the heat in the chauffer not being sufficient to effect the decomposition of the steam, and the consequent combustion of its gaseous inflammable matter.

From what has now been said, I conceive that those engaged in processes where fuel is used extensively, would be justified in making trials on a large scale, how far there may be a saving in the use of steam; and should any be inclined to try it, I would suggest the introduction of steam of *ordinary* pressure at different parts of the furnace, at the *same time admitting air freely*. There is also another suggestion which I would offer; it is, that if there is any benefit derived, it would be worthy of trial, whether or not there would be any advantage by passing the steam through pipes imbedded in the furnace, so as to raise its temperature considerably before admitting it to the fuel, as in the process of using heated air, recommended by Mr Neilson of Glasgow, and now in general use in the iron-works of this country.

On the Possibility of Elucidating the Natural History of Man by the Study of the Domestic Animals. By M. ISIDORE GEOFFRAY SAINT HILAIRE.

OF all the different branches of Zoology, without doubt the most interesting to man is the natural history of man himself. Hence the unceasing, and we may add, ever-increasing zeal which travellers, naturalists, and medical men of every epoch and clime have bestowed in accumulating a multitude of facts and observations, and hence the fresh additions which are daily making to the mighty store. Were the perfection of any science to be estimated by the number of facts it had amassed, no doubt could remain that anthropology would be one of the most advanced of all; but if, on the other hand, less importance is to be attached to the mere number of the observations than to their scientific value, if, in short, facts are to be weighed rather than counted, then we shall be forced to adopt a very different conclusion, and must even avow that there are few branches of Zoology which have not made more marked and decided progress than the natural history of man.

This may, perhaps, be regarded as a singular and parodoxical circumstance, and in fact a great anomaly in the history of the science, but it is nevertheless an incontestible truth, of which the proofs are only too numerous. Observations which, generally speaking, are very imperfect, ill arranged, and but slightly associated, and yielding only uncertain results; or, in other words, materials which may become useful for the future, but which now are scarcely even the elements of an established science, are the only products which can be derived, according to accurate criticism, from the treatises concerning man which have been published up to the present time. It is also true that although zoologists have succeeded in establishing among the innumerable beings which are the objects of their study, divisions of all sorts, which, for the most part, are accurately characterised, and happily linked together, and have thus almost succeeded in establishing a classification for the whole animal kingdom, which is at once natural and logical, they have not yet succeeded in determining with any degree of precision the several types presented by the human race, and of course have not been able to describe them, with a few rare exceptions, in a manner that is at all satisfactory.

Should any inquire respecting the causes to which we attribute this imperfect state, this prolonged infancy, as it may be called, of the science of anthropology, we answer we are far from ascribing it to any deficiency of zeal or ability in those naturalists who have cultivated this branch of zoology; on the other hand we readily allow that generally they have done for it all that was possible. It is the immense difficulty of the subject which has prevented their labours from assuming that precision and scrupulous accuracy which alone would entitle them to a place among scientific results.

The natural history of man, like all other branches of physical science, comprehends results of two kinds, viz. particular facts, obtained by direct observation, and general facts which are deduced by reason from that observation. In a word, the facts are both positive and speculative, and must in fact present this twofold character; facts of the first kind without the second would be only premises without conclusions, and facts of the second without the first would be consequences without premises.

The study of the characters of the several races of men is one of the principal portions of the true natural history of man. Thanks to the labours of a vast number of observers, in the first rank of whom we place the commanders and naturalists of our recent and memorable scientific expeditions, the population of a great part of the surface of the globe has recently been described in a manner more or less accurate. But even when this immense labour shall have thoroughly been executed in regard to all the races of our species, and when even the innumerable variations of their forms, tints and stature shall have been studied, represented, and described, by competent observers, how many obstacles will still obstruct, ere the thousands of facts which are the fruits of these long and painful labours shall be arranged in a satisfactory manner; and still more ere a rigorous determination and an accurate classification of the several types of the human race, will in the end supply a solid basis for anthropological investigations! The immense advance which zoological science has made during the last forty years, may be dated from the epoch when the establishment of so many great scientific museums in so many parts of the civilized world, permitted zoologists to substitute for the analysis of descriptions which are still unsatisfactory even when they are most precise, the examination, direct and comparative, of the very objects themselves which are the subject matter of their studies. The time, we much fear, is still far distant ere such fertile sources of investigation will be at the disposal of the anthropologist; it will be difficult to triumph over the physical obstacles which have a tendency to arrest his progress, and more difficult still to overcome those which superstition and national prejudices almost every where present.

Even under such favouring circumstances, as but rarely present themselves to the anthropologist, when he wishes to supply an account of the analogies and the differences of two or more types, he is almost always confined to a simple comparison of descriptions and figures, which are sometimes untrue, and almost always vague. If the precise and specific characters of two species of an animal often disappear, and, as it were, fade away in description, to such an extent that an able analysis only, elucidated by the direct comparison of analogous objects, can alone apprehend them, how can the anthropologist, deprived as he is of all means of direct comparison, discover in the description of two neighbouring types the slight differences which alone distinguish them? These differences are often in truth nothing more than fleeting shadows which are almost inappreciable, and we should have said beyond the power of expression, had not some recent authors, among whom we name Mr Edwards, demonstrated by their example, that whatever can be determined by observation may be also clearly expressed by words, and had thus revealed to us what may be designated the art of anthropological description.

If the progress of the positive portion of the natural history of man is thus hindered by so many formidable obstacles, it is evident that as great difficulties present themselves to the advance of the theoretical department, for the former is the only and essential basis of the latter; and facts which are imperfectly known, can lead only to imperfect consequences, that is to such as are both dubious and unsatisfactory.

Truly, in this department of science, for one well established truth we find nearly ten assertions which are purely hypothetical, and often directly contradictory. Even since the remarkable labours of M. Bory de Saint Vincent, and of many other anthropologists, those very questions which have been so often discussed, viz. Whether one or several types exist in the human race; and what are its principal varieties,—these questions, we say, with which all others are connected, and are subordinated in the most necessary and closest manner, are not yet solved, or at least not at all in a satisfactory manner. In fact, if you open the books on the subject, and subtract those which only copy Blumenbach and Cuvier, if you reckon only original works, then you will find precisely as many solutions of the chief problems involved, as there are authors; and when there is such contrariety of sentiment, it is but too manifest that truth does not pe vade the science; for it always supplies agreement, simplicity, and conviction, so soon as its demonstration is complete and satisfactory, and almost always ffords this threefold character.

A fresh examination, therefore, of nearly all the questions involved in the natural history of man, and an almost complete revision of the science of anthropology, is imperiously required by its present condition; and these are the only terms on which its future progress can be expedited. This immense labour, the complete success of which is scarcely possible at present, and which is far beyond my feeble power, is not what I purpose to undertake in this treatise, but something which is much more special in its object, and much less extensive in its plan. Once more to review, and subject to a fresh examination several questions which have already been discussed by anthropologists, the satisfactory solution of which may be advanced by the help of the new resources of the science;—to introduce into their discussion some data which have hitherto been neglected;—and, finally, proceeding upon this additional foundation, to substitute in a variety of particulars demonstrated conclusions for hypothetical opinions, and sometimes probable results for simple conjectures; such are the objects I shall endeavour to accomplish in that work of which this memoir forms the first part.

The elements which are usually employed for the determination of the several problems embraced in the natural history of man are, first, the direct comparison of the characters of the races; and, secondly, the comparison of their languages, customs, traditions, of their monuments of every kind, and the circumstances of their *habitat.* Without doubt these are so many excellent sources of information, all of which have already contributed to enrich the science with numerous and interesting results, and which still promise to yield an ample harvest.

But however valuable these elements may be, they are not always adequate to the solution of the difficult and complex questions which occur in anthropology. When they alone are employed, it too frequently happens that even the best directed efforts only supply a glimpse, a mere indication, rather than a demonstration of the important result; or even that they completely fail before the as yet insurmountable difficulties. And if this be the case, should we not seek in the consideration of facts which have hitherto been neglected, and in their application to the yet unsolved problems, the means of introducing new elements into the discussion, thereby to obtain new instruments for arriving at a satisfactory determination.

These new elements and new methods of solution I have found, by applying to the history of man different facts, some of which are little known, but the majority of which are familiar and almost trivial, drawn from the history of the domestic animals. It is not, then, by anthropological facts that I now proceed to illustrate anthropology, but by considerations borrowed from a collateral branch of science; thus substituting for the usual methods, or rather bringing to their help, a method which it must be allowed is less direct, and whose employment, from that circumstance alone, might appear more difficult. But this inference is of no great moment, if it be found that the method happily leads to our object, and if we can thus sometimes succeed in arriving by a circuitous route, at a result which we cannot reach by the more direct course.

It may besides be supposed by many, that the variations of the domestic animals, and those of the human race, have betwixt them only such distant and indirect connections as a first superficial examination might suggest. This is far, however, from being the case; and we shall speedily find that there result from these data, I do not say only intimate, but also twofold, connections, viz. links of analogy, and links of causation: of analogy, because the variations of mankind, and those of the domestic races of animals, follow the same laws, and exhibit similar characters; and of causation, because the various modifications of our different domesticated races are produced by the influence of man, variously modified by time, place, and circumstance. Thus, as may at once be perceived, the consideration of the races of the domestic animals, when introduced into the discussion of anthropological problems, will elucidate them by data of two kinds, and from this single, but twofold, element two fertile sources of induction flow.

Let us first examine the relations of analogy which exist between the variations in the races of domestic animals and those of the human race; let us endeavour to ascertain their nature; and so far as possible to determine, and, so to speak, to measure their value.

When we compare different individuals of a wild animal which have been captured in regions which vary as to temperature, topography, and generally as to every thing comprehended under the name of local circumstances, when we subject these individuals to a sufficiently attentive examination, we always succeed in discovering that there are differences more or less marked among them. Except in the case which is altogether foreign to our subject, of an accidental or *teratological* modification,* the different traits of each of the individuals selected as types of comparison are otherwise very far from belonging to it alone; they are likewise discovered in all the individuals which live in the same country, and the same local circumstances, and are regularly transmitted by procreation; they therefore characterise hereditary varieties, or, in other words, and precisely in the same sense in which the word is employed in regard to man and the domesticated animals, *races.*

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* Considered in the general, and not as I am now doing in a particular, point of view, the question of the origin of races is very intimately allied with various important questions of *teratology*, as I have shewn in many passages of the 1st and 3d vols. of my *Histoire Générale des Anomalies de l'Organisation.*

The distinguishing characters of races, which relate principally in the majority of cases to their colour and dimensions, are in some species very marked and manifest at the first glance; in others they are observed with greater difficulty, and sometimes can scarcely be discovered. These differences render the general fact I have just been pointing out somewhat more difficult to determine, but they do not invalidate it; and an explanation of them all may readily be deduced from considerations which are in themselves sufficiently simple. In truth it is sufficient, on the one hand, to think of the very many and great variations which the animals present in their mode of life, and in their habitat, to enable us to perceive that they should not all be affected in the same degree by the influence of climate, the topographical position, and the other local circumstances of the country which they inhabit. On the other hand, observation reveals to us another cause which is somewhat more difficult to foresee by means of reasoning, and which consists in differences even of organization; it is, however, true that some types resist more, and others more readily yield to the influence of local circumstances, even when these last are, or at least appear to be, precisely the same in both cases.

To this opinion, then, that the wild species are variable under the influence of different local circumstances,—that there exists among them hereditary varieties or races as among domesticated animals; it is moreover necessary to add this other consideration, viz. that they are variable in unequal degrees. Both of these opinions are alike incontestible. But this inequality should not prevent, and in fact does not, the existence in the limits of variation peculiar to each species, a well determined relation betwixt the intensity of the modification and that of the differences under the influence of which they are produced. Here, as in every other case, the effect is in the ratio of the cause, and observation as well as theory authorizes us to regard in the wild varieties the differences of the races as proportioned, other things being equal, to the difference of the circumstances in the midst of which the races subsist.

The application of these sentiments regarding the hereditary varieties or races of wild animals, to the hereditary varieties or races of domesticated animals and to man, is both direct and easy. All the modifications, so varied, so complex, and, in appearance, so unintelligible, which these latter present, are the same modifications which wild animals exhibit, only produced on a more extensive scale; the same causes which produce the former produce also the latter, although they are increased both in number and in intensity.

Unless a wild variety exists at one and the same time in positions which are very different as to their elevation, and consequently as to their temperature and atmospheric pressure, an event which but rarely happens; and unless it is also found widely distributed in some places which are very arid, and in others which are very humid, a coincidence perhaps rarer still, we must, ere we can find a species with marked differences, necessarily take for the terms of our comparison, individuals which belong to regions which are widely separated from each other. But this possibility is itself included within a determinate and generally a very narrow circle. The geographical distribution of each being is rigorously fixed by its necessities and requirements; wherever local circumstances which are very different, might lead to important modifications in the organization of a species, and especially because it is so, there, that species is no longer to be found; because, free to range at will, it wanders where circumstances are favourable to it, that is to say, to those localities which, agreeing with the requirements of its organization, tend to preserve its type, and not to modify it by a powerful, and on that very account, a very troublesome reaction.

The conditions of variation are very different in domesticated animals. In the first place, very marked modifications are observed without a proportionate difference in the region they inhabit; for the omnipotence of man, acting variously on the animals which are subjected to him, creates for them in the same region local circumstances which are entirely dissimilar. In the second place, the number and the intensity of modifying influences become, so to speak, illimitable, for we no longer have for any domestic breed either determinate nourishment, habits, or climate, and as often as the will of man acts upon them in a different way so often are they placed under new causes of variation.

Thus, precisely and for the same reasons, we find so many varieties transmitting themselves hereditarily in the human family. Residing in every clime, and almost under every degree of temperature, changing in a thousand ways the quality and the quantity of his food, and following the most different kinds of occupation, he presents in the multiplicity of his races and subraces, and we might add, of his innumerable individual varieties, the natural and necessary effect of the multiplicity of the causes which operate upon him, and have for so long a period exerted all their influence.

Thus it happens, on the one hand, that among wild animals, we find the causes of variation restrained within very narrow limits, and consequently the varieties but few and not well marked; on the other hand, among domestic animals, and in man, who in this point of view must be assimilated with them, there are many causes, and consequently many manifestations of variation, whose limits, both in number and power, can scarcely be traced. But if, in this respect, there exists this vast difference between these two classes, it is easy to see that the state of civilization in man, and the domestic condition which so exactly corresponds to it among animals, have not really created a new order of causes and effects, but have only multiplied, augmented, and varied in detail the causes and effects which already existed among wild beasts. In both, the modifying influences are always the local circumstances, and more especially the habitat, the kind of life, and food; and the effects of the variations, first as it respects the size and the colour, and then the proportion and form of the organs; a double resemblance which I might trace into very minute details, and of which I might supply long and accurate, and at the same time rigorous demonstrations, if the preceding remarks, and the evident confirmation they receive from a variety of well known facts, did not render it unnecessary.

The benefit which may be deduced from these considerations in the promotion of the special objects of this treatise is, as we shall presently see, both direct and important. If the physical variations which are produced in man by the influence of his civilization were phenomena of a peculiar kind, and if the human race were found in this, as in so many other respects, to be beyond the range of the lower creation, it is clear that, in the study of the human races, we should be reduced to the necessity of not ranging beyond the limits of the circle of anthropological facts; every fact borrowed from the other branches of the science would only be a source of error, and nothing more. But if the physical variations of man exhibit manifest relations to the variations of animals, if they consist in similar effects which are explicable by the same causes, and reducible to the same laws, then analogy becomes, in the study of the races of mankind, a guide as useful, as in the former supposition it was dangerous. And, finally, if we are led to recognise that these same physical variations in man, generally analogous in their nature to the variations of the different races of animals, are, in particular, exactly and in every respect legitimate objects of comparison with those of the domestic breeds, the study of these races of men, and that of the domestic races of animals, manifestly becomes a reciprocal and necessary complement to the other; and to isolate them is nothing else than to suppress among the data of the difficult problems to which they relate, one-half of the elements which might and ought to contribute to their solution.

Thus have we indicated one class of applications which have hitherto been almost entirely neglected, although the relations whence they are derived have been for a long time noticed, but in a very confused manner. We now proceed to a second series of applications which have still more completely been overlooked, and the very principle of which has scarcely been recognised in the science.

With this object in view, let us for a few moments consider, in the abstract, the analogy, on which we have been dwelling, which exists betwixt the variations of the human race and those of the domestic animals; and without resting either on the nature of the changes in these latter, or their mode of production, let us confine ourselves to the consideration of the effects, in so far as they relate to the general cause.

The variations in the domestic races are of two kinds; 1st, Variations of the races in relation to the wild or primitive race; and, 2d, Variations of the races as among themselves. Both of these have been attributed from the commencement of the science to the influence of domestication, and the remarks which we have offered above suffice to establish that this explanation is as just as it is old. Moreover, it is beyond dispute that the influence of domestication is nothing more than the influence, sometimes direct, sometimes indirect, of the power of man, subjecting to his sway those species which contribute to his nourishment, his industry, and his pleasures, and in this manner creating for them conditions which are very different, indeed, from those of their wild and primitive state.

Considered in this point of view, the domestic animals themselves, therefore, may truly be reckoned among the handiworks of man; they exhibit in all those modifications which separate them from their primitive types so many unexceptionable traces of the influence and power of our race in ages that are past; and are, in a word, if I may so express myself, monuments of a particular kind, but at the same time as durable as any of those which more commonly go under the name. Is it not in truth man who has made the dog what he is, and not less the horse, the sheep, and many other animals under our eye; that is to say, who, subjecting them to his yoke in a very remote epoch, whose date is almost invariably lost in the obscurity of time, has successively modified these useful animals, has developed in them new faculties and instincts, new at least when compared with their primitive condition, has moreover imprinted on them those forms and characters which they at present bear, and, finally, from the one spot on the face of the earth where nature had fixed their habitat, has transported them, and spread them over the known regions of the civilized world.

Thus, as it regards organization, instinct, habits and country, man has modified all in the domestic varieties, every where bending and subjecting the primitive type to the law of his wants, his wishes and inclination; an immense work this, both in itself and its results, at once the first proof and the first foundation of this almost illimitable power and industry.

From these important relations of causation betwixt man's power universally exercised according to time, place, and circumstance, and the different modifications of the domestic animals; —from these alliances betwixt two classes of actions and phenomena, which at first view might be regarded altogether foreign to each other, there manifestly results the possibility of elucidating the study of the one by that of the other; and hence this second and valuable source, whence we can procure other and not less valuable applications to the science of anthropology.

But, in truth, our reasoning demonstrates only the general and absolute, and not the present and immediate possibility of such applications; and it might have happened that the present state of the science, whilst it promised them for the future, denied their present realization. Happily, however, this is not altogether the case, and even now we can, by a searching examination of several questions, deduce corollaries, the number and importance of which will necessarily increase with the future progress of zoological science. Thus, to cite some examples, may we not easily conceive, at least in a general way (and already important researches have been made by M. Dureau de la Malle with this object), that the determination of the *original* habitat of species, which are now nearly spread over the whole surface of the globe, might supply views respecting the first place of their domestication, and consequently throw some light upon the ancient relations of different nations? And may we not also perceive, that when in any way we fix the relative order of the domestication of species, a task which we can now perform for several, we may then arrive at useful deductions concerning the relative period of the civilization of different tribes? And, finally, is it not evident that the ideas promulgated by different authors concerning the analogies and discrepancies, the common or the different origin of certain tribes, may be confirmed or invalidated, at all events, in some cases, by the comparative study of their domestic animals, as well as by that of their languages, and of their monuments of every kind.

Such are the ideas upon which I shall be enabled to found some new and useful inductions applicable to the natural history of man. All of them flow, directly or indirectly, from the theory of the modifying influence which is exercised by local circumstances upon living beings; a theory which is nearly sterile, if it be judged by the small number of results it has hitherto produced, fettered as it has been by a powerful, but happily not invincible opposition; and a theory, on the contrary, hich will be eminently productive, if we measure it with a lively prehension of all the progress which its definite admission into e science is calculated to produce.

All simple as the ideas propounded in this essay may be, it appeared necessary to discuss and develope them ere I proceeded to the corollaries which I mean to deduce from them. The intimate links which connect these ideas, and consequently the corollaries also, to a theory which had been long disputed, and very generally misunderstood, made this preliminary labour the more necessary. Moreover, it is only reasonable, and almost indispensable, that when one is about to employ a new or but little known instrument, he should first carefully consider all the advantage he can derive from it, and so to speak, estimate its power. Such was my aim in making these remarks, preliminary to a memoir I shall speedily submit to the Academy upon a long contested question, THE SPECIFIC UNITY OF MAN.

On the Sivatherium, a new fossil Ruminant Genus found in Tertiary Strata in the Valley of the Markanda, in the Sivalik branch of the Sub-Himalayan Mountains.

THE fossil remains of this new genus were discovered in the district above mentioned, associated with bones of the fossil elephant, mastodon, rhinoceros, hippopotamus, &c., by two meritorious officers, Dr Hugh Falconer and Captain P. T. Cautley, who have published an interesting account of their investigations in the First Part of the Nineteenth Volume of the Asiatic Researches, printed at Calcutta in 1836. From the details of these officers, it cannot be doubted that the Sivatherium belongs to a genus now, for the first time, made known to naturalists. Some difference of opinion has arisen as to the place which this newly discovered animal should occupy in the zoological system. Messrs Falconer and Cautley remark,

> "That the Sivatherium was a very remarkable animal, and fills up an important blank in the interval between the Ruminantia and the Pachydermata. That it was a ruminant, its teeth and horns clearly establish; and the structure which we have inferred of the upper lip, its being a proboscis, the osteology of the face, and the size and position of the orbit, approximate it to the Pachydermata. The circumstance of any thing approaching a proboscis is so abnormal for a ruminant, that, at first view, it might raise a doubt regarding

the correctness of the ordinal position assigned to the fossil; but, when we inquire farther, the difficulty ceases. In the Pachydermata, there are genera with a trunk, and others without a trace of it. This organ is therefore not essential to the constitution of the order, but accidental to the size of the head, or habits of the animal, in certain genera. Thus, in the elephant, Nature has given a short neck to support the huge head, the enormous tusks, and the large grinding apparatus of the animal; and, by such an arrangement, the construction of the rest of the frame is saved from the disturbance which a long neck would have entailed. But, as the lever of the head becomes shortened, some other method of reaching its food becomes necessary; and a trunk was appended to the mouth. We have only to apply analogous conditions to a ruminant, and a trunk is equally required. In fact, the camel exhibits a rudimentary form of this organ under different circumstances. The upper lip is cleft; each of the divisions is separately moveable and extensible, so as to be an excellent organ of touch."

Geoffroy de St Hilaire, on the contrary, maintains that it belongs to the Giraffe tribe, an opinion rejected by M. de Blainville, who remarks that there is no other resemblance between the Sivatherium and the Giraffe than that which belongs to animals of the same order of ruminants, as is evident on comparing the figure of the head of the Sivatherium, plate ii., as given by Messrs Falconer and Cautley, with that of the Giraffe, in plate ii. In this comparison, M. de Blainville notes the following particulars:—

"1*st*, In the *Sivatherium*, the general form of the head is angular, we might almost say triangular, very large behind, with an elevated vertex; and, on the contrary, much contracted and attenuated before, and exhibiting only two depressions, the one of no great depth, behind the orbits; and the other very conspicuous before the molars. The upper mesial line ascends very rapidly from the anterior extremity to the posterior; and the inferior, on the contrary, starts up at an acute angle from behind the maxillary towards its vertebral portion, somewhat as is observed in the rhinoceros, so that, if the head be placed on a plane resting upon the teeth, the condyles of the occipital are raised far above that plane. In the Giraffe, on the contrary, the head is long and straight, with an almost uniform slope throughout the whole length, both in the upper and under mesial line, so that the two extremities are scarcely raised from any plane on which it lies. Its greatest breadth is besides from side to side, and not behind, but near the middle, in the diameter of the orbits, whence it becomes smaller, both behind and before.

2*d*, In the *Sivatherium* the occiput, or rather the vertex, is exceedingly remarkable, inasmuch as, in addition to its great height, it also bulges out on each side into a considerable protuberance, so much so that Dr Falconer and Captain Cautley imagined that these protuberances may have been prolonged into horns; were this the case, then it would follow, that, as in oxen, the posterior enlargement of the head would be formed by the frontal bones,—a sup position somewhat confirmed by the additional remarks of these gentlemen that the cranium, though mutilated in its parietal, region, appeared, in the junction of the parietals with the frontal, to resemble that of the ox. In the giraffe, on the contrary, the occiput is rather contracted than dilated, and shews no appearance of the lateral protuberances of the *Sivatherium;* again, the frontal, which in part supports the epiphyses of its false horns, is far from reaching the occipital region of the head.

3d, The forehead is especially very remarkable in the *Sivatherium,* not only by its breadth, and by the deep excavation of its upper part, but still more in that between the orbits, and somewhat above and behind them; there springs from a broad base, and insensibly from the frontal, two large additional protuberances which are short, conical, smooth, and with ridges which diverge from each other, and project obliquely forward. On the other hand, there is nothing the least corresponding to this in the giraffe; and, on the contrary, its forehead, instead of being broad and excavated, is rounded, and rises into a kind of mesial crest, or compressed protuberance, which supports the mesial horn-shaped epiphysis so characteristic of this animal.

4th, As it regards the processes or prolongations with which the cranium is supplied, there could, if possible, be still less resemblance, whether we refer to their number, their position, or their structure. In the *Sivatherium* true horns unquestionably existed, for the bony prolongations are continued without interruption into the frontal bone; and if in the figure it appears that the stump of the right side is separated by a suture, yet on more minute examination it will be found that this is truly a partial fracture, and that there is no trace of the appearance on the other side. Besides, these horns were either to the number of two or four, two being supra-orbital, and two sub-occipital, as in the *Antelope quadricornis.* In the giraffe, on the other hand, there are no horns properly so called, but the skin uplifted, so to speak, in two or three places, according to the sex, is supported by peculiar epiphyses, which, though vascular, are always full, and have more resemblance to the branches of the stag than to a horn, being always more or less hollow, and having a communication with the frontal sinuses. Besides, these frontal prolongations are in the giraffe three in number, one quite mesial in the centre of the forehead, and the others placed laterally on the fronto-parietal suture.

5th, The orbits in the *Sivatherium,* as in the giraffe and all the ungulata, are widely distant or separated from each other; but they are moreover very small in the first named animal, and the plane of their aperture is altogether lateral, whilst in the other they are very large, and antero-lateral, a difference which must have very conspicuously altered the appearance of the animals.

6th, In the *Sivatherium* the face is short, broad, and massive, somewhat resembling what we see in the elephant; whilst in most other ruminants, and in the giraffe in particular, it is altogether the reverse.

7th, The bones of the nose are short and arched, and project much beyond the posterior margin of the nasal fossa in the *Sivatherium,* which makes it somewhat resemble the rhinoceros, and especially in the figure, on account of the mutilated state of the muzzle; whilst in the giraffe, on the contrary, these bones are very long and broad behind, and attenuated and bifurcated before, scarcely extending beyond the posterior origin of the nasal fossa.

8th, The zygomatic arches are not at all prominent; they are long, and project forward to join the corresponding process of the cheek-bone, (jugal), a construction which is likewise found in the giraffe and other ruminants; but which, we may remark in passing, in no respect agrees with the figure, which represents the zygomatic arch as broad, thick, and having a certain resemblance to that of the rhinoceros; it ought, however, to be added, that the parts are not very distinctly seen.

9th, Finally, The molar teeth, six in number, as in all the ruminants, and having a very close resemblance with those of the camel, are notwithstanding much more thick and broad; and besides, the posterior three exhibit the form of a cross at the inner side of their triturating surface, which, in place of simply bending, is arranged in zig-zag, or in deep sinuosities, somewhat as in the *Elasmotherium*, and even in the *Anoplotherium*, a construction to which there is nothing similar in the giraffe, nor in any other ruminant with which we are now acquainted.

In conclusion, I may remark, it would be a very easy matter to adduce other proofs of the inaccuracy of the propositions of M. Geoffroy, extracting them either from the memoir already quoted, or by farther extending the comparison between the head of the *Sivatherium* and that of the giraffe; but, to cut short the discussion, I will only again refer to the copy here given of the figure supplied by Messrs Falconer and Cautley, which has been taken from a head somewhat mutilated both before and behind, of which all the sutures, being completely united, demonstrates that the animal was more than adult, and will contrast with it that of a giraffe, which is much reduced in size. I trust that this will at once demonstrate that the *Sivatherium* was really a most extraordinary looking animal; in short, an immense kind of antelope, still more hideous than the *Gnu* (*Antilope gnu*, L.), with a short and heavy head, with an elevated cranium, and very broad especially behind, sustaining probably two pair of horns, one, the smaller set, in front, and the other quite behind as in the aurochs, with the face and figure of the rhinoceros, having small lateral eyes, and undoubtedly with great lips, and perhaps even a nasal proboscis, as Messrs Falconer and Cautley imagine; and whose neck and extremities must have been in proportion, that is to say, robust and strong, and far from high, which in all respects is the very opposite to what exists in the giraffe; an animal the whole of whose organization, including its relative proportion and its peculiar gait, indicate the inhabitant of a vastly extended country of plains and forests, and not at all of a mountainous region*.

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* Messrs Falconer and Cautley, in the valuable volume of the "Asiatic Researches" above quoted, give an interesting description, with figures, of the fossil remains of a Camel which they consider to be a different species from the Bactrian Camel and Dromedary; they assume it to be extinct, and name it *Camelus Sivalensis*. They further describe a new fossil Tiger, (their *Felis cristata*), size of the common tiger; a new sub-genus of Hippopotamus, of which two species are described; a gigantic but new species of Ursus, their *Ursis Sivalensis*, and new fossil Crocodile, and fossil Ghavial—all from the Sivalik tertiary Hills. In the September number for 1836 of Journal of Asiatic Society of Calcutta, there is a series of notices of "*Smaller fossil Carnivora*," found in the Sub-Hamalyas by Messrs Barker and Durant. They describe, as probably extinct animals, species of the genera *Felis*, *Gulo* and *Vulpes*.

Respecting some new Researches on the well-known Phenomenon of the Erosion of the Columns of the Temple of Serapis, at Puozzoli. By M. CAPOCCI, Director of the Observatory at Naples.

M. ARAGO, at the meeting of the Academy of Sciences on the 15th of May last, made a verbal report, as he had been requested, concerning the relative changes which the level of the sea and the coast appear to have undergone in the neighbourhood of Puozzoli.

M. Capocci states that M. Nicolini, his fellow-countryman, had established on satisfactory documents, the following propositions :—1st, That the epoch (anterior to our common era), when in the Temple of Serapis, the Mosaic pavement was constructed, which has been discovered under the more recent pavement of marble, the level of the sea in that locality, *compared with that of the land*, was *lower* than at the present time by fifteen Neapolitan hand-breadths (the hand-breadth is about 0,8515 English feet). 2d, That, in the first centuries of the common era, which corresponds to the time when the warm baths and the new pavement were built, the level of the sea was six hand-breadths and a-half *above* the present level. 3d, That in the middle ages the level of the waters was about twenty-two hand-breadths above the present level; and, 4th, That at the commencement of the present century, the sea was *lower* than now to the extent of two and a-half hand-breadths.

In support of the opinion which attributes these movements to the soil, and not to the sea, M. Capocci cites, and this is the most important part of his memoir, various passages taken from the accounts of eye-witnesses of the terrible eruption which, in the year 1538, produced a new mountain, the famous *Monte Nuovo* near the lake Lucrino. All these writers, viz. Porzio, Toledo, Borgia, and the second Falconi, agree in delaring that *the sea retired from the shore to the extent of* 200 *paces.** But how is it possible that the sea could retire, thus lowering its level, and that permanently in one point of the gulf, without lowering its level and retreating at the same time at the neighbouring places? and yet it is certain it did not retire either at Naples, or at Castellamare, or at Ischia. In 1538, therefore,* it was the shore which in a single locality elevated itself, and was found dry. However, we shall subjoin the precise words of Porzio, a man of uncommon genius and profound knowledge, and characterized by his contemporaries as the prince of the philosophers of the day. "This region," he states, "was agitated for nearly two years by violent earthquakes, to such an extent that none of its houses remained uninjured, nor a single edifice which was not threatened with an inevitable destruction. During the fifth and fourth day before the kalends of October, the earth trembled without ceasing, night and day; the sea retired about 200 paces; and on the dry shore the inhabitants took a multitude of fish, and remarked springs of fresh water spouting up. Finally, the third day before the kalends, a large portion of the ground between the foot of *Monte Barbaro* and the sea near the Averno, appeared to rise up, and assume the form of a nascent mountain. This same day, at the second hour of the night, this upborne ground transforming itself into a crater, vomited forth, with violent convulsions, torrents of fire, showers of scoriæ, stones and ashes!"

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* Lofredo stated in 1580, that fifty years before that date they were in the habit of fishing on spots where in his time they saw old ruins between Puozzoli and Lucrino.

These words seem to leave no doubt as to the elevation of the soil, unless indeed we should wish to support the subtile explanation furnished by the author himself, in a passage we shall now quote. "The sea retired at first, solely no doubt because the exhalation requiring an issue burst asunder and separated different parts of the earth, and thus rent, it then absorbed the water by the small fissures; whence it happened that this portion of the ground, which had been hitherto covered by the sea, remained dry, and the shore was raised by the accumulation of cinders and stones." But, in the very face of a visible elevation of a part of the ground, "*magnus terræ tractus . . . sese erigere videbatur*," why go in search of a complicated and difficult explanation, in which it is, besides, difficult to discover how the water, which was so very rapidly rushing down through the crevices, did not carry away with it the cinders and stones, but left them to accumulate and raise the level of the shore? And this elevation was not inconsiderable; for the soil, according to the measures quoted, anteriorly to the year 1538, must have sunk nearly to the extent of 22 hand-breadths under the real height: at the commencement of the present century it was above that real height, to the extent of 2½ hand-breadths. The total rising then, in 1538, could not be less than 24 hand-breadths,—a limit which it probably exceeded, since the descending movement, which is remarked in our own day, could not have commenced only of late years. M. Capocci also examines whether the ground had changed its level for some distance along the coast, and finds that the uprising must have extended from the spot where the ancient baths of mineral water had been re-established, as far as the stoves of Nero. To the east of the baths, near to Nisita, and more to the west than the stoves, near to Baia, the ground seems to have maintained its level, or perhaps even to have somewhat sunk. In truth, in different places within these limits it is found that the water rises above the ruins of ancient buildings,* particularly at Baia, near the temple of Venus. Besides, we cannot here discover along the shore, at any distance from the water's edge, the slightest trace of a former water line, as may be seen in the intermediate space, and chiefly from Puozzoli to the lake Lucrino. In this intermediate space, and exactly about 200 paces from the water's edge, the land presents, all along the road formed posteriorly to 1538, a kind of projection, against which it would appear that the waters had formerly beat. This projection, therefore, which is not connected with the present shore by any gradual slope, indicates a sudden change, and not a gradual displacement, in the line of the coast.

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* The Temple of Serapis was in 1538, like Pompeii, buried to a certain extent, which circumstance prevented the three columns which remained erect from being perforated at their lower part.

The fact reported by M. Capocci, that, since 1800, the sea has appeared to subside 2½ hand-breadths in the neighbourhood of Puozzoli, seems well worthy of a serious examination. Let us hope, remarked M. Arago, that the Neapolitan Government, which has so magnificently supplied the necessary instruments to the new Observatory of *Capo di Monte*, and more recently still, has authorised M. Capocci to enrich the scientific institutions of Naples with the first-rate instruments of every kind which can be manufactured in France, England, and Germany, will likewise supply to this able astronomer the means of prosecuting with assiduity so highly interesting a phenomenon connected with the physical history of the globe. The annual observation of the levels, together with thermometrical observations taken at great depths, would besides shew the estimate we should form of Mr Babbage's ingenious idea; according to which, the variations on the surface of the earth, noticed in so many places, may be owing to marked local changes of temperature in the lower strata of the earth. Mr Babbage calculates that a change of 100° Fahr. affecting a formation of sandstone to a depth of five miles, would cause a movement upwards at the surface to the extent of twenty-five feet.

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* There are also at Puozzoli some buildings which are submerged; but here it is evidently the exception, whilst within the other limits it is generally the case.

Considerations concerning the Manner in which was formed in the Mediterranean, in July 1831, *the New Island which has successively been called Ferdinandia, Hotham, Graham, Nerita, and Julia.* By M. Arago.

M. Arago, after having given an account of the memoir just noticed, in which M. Capocci establishes upon most important historical documents, that at the epoch of the formation of *Monte Nuovo* there had been a considerable elevation of all the surrounding grounds, communicated to the Academy, as a supplement to his report, the considerations which have led him to think, contrary to the almost universal opinion of geologists, that in its submerged portions, at least, Graham or Julia Island was caused by the upraising of the solid and rocky bed of the ocean.

These considerations are of two kinds; both of which we shall cessively analyze. In examining the *Nautical Journal* of Lapierre, the commander of the brig *La Fleche*, M. Arago found a number of soundings taken round the new island, on the 29th September 1831. With these observations M. Arago was able to calculate the mean inclination, in reference to the horizon, of the immersed portion of the islet, comprehended between its shore, and the corresponding points at which the soundings were effected. The following table contains these results, and the calculated inclinations :—

Distance of Sounding Line from the Shore.	The Depth.	The Calculated Inclinations.
40 fathoms to the N.	52 fathoms	47¼°
20 ... ... N. E.	43 ...	62½°
30 ... ... E.	52 ...	55⅓°
30 ... ... S. S. E.	50 ...	54¼°
30 ... ... S. S. W.	50 ...	54¼°
30 ... ... W.	42 ...	49⅓°
30 ... ... N. W.	45 ...	57⅓°

Other observations and other calculations, which we shall not adduce here, give to the *emerged* sides of the new island, declivities which diminish as they recede from the shore ; and the variation is even considerable. It appears, then, evident, that if M. Lapierre, instead of sounding at horizontal distances from the coast extending to 30 and 40 fathoms, had taken them at 8 or 10 fathoms, he would have found inclinations amounting to 70, and even perhaps to 75 degrees. I leave it, said M. Arago, to those who have most attentively studied the configuration of the globe to decide if moving ground, without cohesion, and incessantly beaten by the waves of the ocean, if ashes, and above all small stones, in the supposition that Graham or Julia Island has been formed from them, could have maintained itself for whole months under such great inclinations.[*]

But a few additional figures will put every one in a capacity to appreciate the value of the remarks which have just been made.

Inclination with the horizon of the slope of the cone of Vesuvius, according to Elie de Beaumont, . . . 33°

That of the slope of the upper cone of Etna, . 32° & 33°

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[*] At the time that M. Constant Prevost gave an account of his interesting investigations, I learned from him that in a certain point, at the distance of 30 or 40 feet from the shore, he found a depth of 200 feet. The two numbers 40 and 200 correspond to an inclination of 78⅔° ; and 30 and 200 yield 81¼°.

Inclination of the most rapid slope of scoriæ in the same mountain. . . . . 37°

The slope according to which very fine dry sand, and powdered sandstone, arranges itself, in relation to the horizon, according to M. Rondelet, is an angle of 34°$\frac{1}{2}$

The angle of the natural slope of common earth dry and powdered according to the same architect, is . 46°$\frac{5}{6}$

After moistening the earth, he found, as the mean of different experiments, that the angle amounted to . 50°

We now proceed to the second class of considerations which M. Arago developes.

The island of Julia became visible between the 28th of June and the 8th of July 1831; all doubt is confined within these limits. In fact, at the former of these dates, Captain Swinburne of the English service, traversed the space comprehended between *Sciacca* on the coast of Sicily, and the island *Pantelaria*, in which the new islet has since risen, and that without observing any thing extraordinary; on the 8th July again, the Neapolitan captain Jean Corrao witnessed the manifest traces of the irruption.

M. Prevost, during his voyage, obtained some information which is most important as it respects the formation of the islet; for Prince Pignatelli assured him, that during the first days of its appearance, as, for instance, on the 10th or 11th of July, the column which ascended from the centre of the island, burned during the night with a very vivid and continuous light; the Prince compared the phenomenon to the appearance of fire-works.

Even at the beginning of August this same column of dust still gave out light, which, if not altogether so strong as the Prince remarked, was at least very visible. Captain Irton and Dr Davy are both vouchers of this fact. On the 5th of August it is true, that Dr Davy being at some distance from the island, in a situation where the impalpable powder carried along by the wind fell in abundance, observed that it was not hot as he received it on his hand; but here we are not to forget the amazing rapidity with which bodies that are very thin and slender, such as burning metallic wires, for example, assume the perature of the air. With this fact in our minds, we shall be disposed to deduce from the remark of Dr Davy the con- ence that all the earthy dejections of the crater, and those even which, falling down vertically, unceasingly augmented the apparent size of the islet, were cold. And besides, it is well known that for two whole months it was scarcely possible to walk upon the island, on account of the heat of the scoriæ and sand of which it was composed.

If the submersed portion of the new isle had been formed by the superposition of incandescent materials, or at least of matters which were very hot, as was the portion which protruded from the wave, it could not have failed to have heated the sea, at all events to a certain distance; thus in approaching the island, a thermometer introduced into the sea should have gradually risen. But this is precisely the reverse of what took place; for the *diminution* of temperature observed by Dr Davy on the 5th of August in approaching the isle amounted to 5°,6 Centigrade.

Dr Davy, struck with this great diminution, conceived it must be attributed to the floating powder with which the sea was covered on the occasion. According to his view the dust projected in the vertical column by the crater, must have acquired, when it fell upon the water, the low temperature which it had acquired in the more elevated atmospheric strata. This explanation, however, seems liable to two serious objections: *first*, it is not very evident why each particle of dust in again traversing the atmospheric strata from above downwards, should not have re-acquired all the heat which it lost in ascending; and, *secondly*, we must remark that the total height of the column was not above 400 feet English, an altitude which, according to the known law of the decrease of atmospheric temperature, would only produce a difference of two-thirds of a degree of the Centigrade scale.

The 5°,6 of refrigeration observed by Dr Davy surpass by a great deal every thing which hitherto has been found on approaching the islands or shallows of the Mediterranean, or even the islands and shallows of the ocean. It is not sufficient then to reject the hypothesis which would have implied an augmentation of temperature; it moreover remains that we should explain how the refrigerating influence of the islet should have been so great. And nothing can be more simple, for we have only to suppose that the island was at first formed in the way of *soulevement* or upraising already suggested—that the highly inclined planes of the submerged portion were the uplifted bed of the sea—that it was composed of strata which had been cooled for ages, and the whole anomaly is at an end.

The following are some additional results culled from M. Lapierre's journal, and which seem to corroborate the preceding views:—

The surface of the ocean was found at a temperature of 73°4 Fah.
At the depth of one fathom it was also found at 73°4
At the depth of ten fathoms it was found to be only 70°0
At the depth of thirty fathoms it was found to be only 67°0

Once more, continued M. Arago, in drawing this verbal communication to a close, these views will probably be brought from the region of simple conjecture, when the whole of the thermometrical observations which have been made in the neighbourhood of the new island shall have been published; and when, in addition, the maximum influence which a small permanent island, similarly circumstanced, exercises on the sea, shall have been determined; as, for example, that of the island Pantelaria. To urge forward these observations and publications is the anxious wish of the learned Secretary.

On the Stony Matters which are employed in China in the time of Famine, under the name of Flour of Stone. By M. Biot.

The details which were communicated to the Academy of Sciences by M. de Humboldt concerning the existence of a stony substance, which is sometimes employed in Lapland, in the time of dearth, have recalled to my recollection the notice of a similar fact which has lately reached us from China, and which was reported in the correspondence of the missionaries. My son having likewise found the same fact, attested at many different periods in the Japanese *Encyclopédia*, with the dates attached, I requested him to translate the passages which related to the subject; and it has occurred to me that the Academy would regard with interest the collection of these documents concerning the employment of the article in a way much more general than we are usually led to believe.

The Japanese Encyclopedia, book lxi., "Upon Stones and Minerals," contains an article entitled *Chi Mien* or *Stone-flour*, of which we now present a translation; and in which it will be seen the same superstitious ideas prevail which M. de Humboldt had remarked in Laponia. "The *Pen-tsao-Kang-mou*[*]" remarks, "The flour of stone is not an ordinary substance, but a miraculous production. Many declare that it is produced in the time of famine. Under the Emperor *Hien-Tsong*, of the dynasty of *Tang*, in the period *Tien-pao*, the third year (answering to A. D. 744), a miraculous spring issued from the earth, and stones were decomposed and converted into flour." To the letter-press of this extract is conjoined a wood-cut, which represents the spring issuing forth in cascades, and the stones breaking up into slender threads, but these are so incorrectly indicated that it is not possible to draw any mineralogical inference.

We subjoin some additional notices. "Under the Emperor *Hian-Tsong*, of the same dynasty, in the period *Yuen-ho*, fourth year (A. D. 809), stones were decomposed and became meal." Under the Emperor *Tching-Tsong*, of the dynasty of *Soung*, in the period *Tsiang-fou*, fifth year (A. D. 1012), "a marrow was produced from stones which resembled flour." Under *Jin-Tsong*, in the period *Kia-yeou*, seventh year (A. D. 1062), "the flour of stone was produced." Under *Tchi-Tsong*, in the period *Yuen-fong*, third year (A. D. 1080), "stones were decomposed and became flour: All these kinds of flour were collected and eaten by the poor."

We now add the statement, made in 1834, by one of the Chinese missionaries, M. Mathieu-Ly, who is established in the province of *Kiang-Si*.[†] The facts which he describes relate to the same year 1834, and to the three preceding, so that they coincide with those mentioned by M. Retzius regarding Laponia. "Many of our converts will assuredly die this year from want; and it is God alone who can provide a remedy for so many and such aggravated necessities; all the crops have again been carried away by the overflowing of the rivers. For a period of three years now, an immense number of persons have supported themselves upon the bark of a tree which is found in the country; whilst others eat a light earth of a white colour, which has been discovered in a mountain. The earth can only be bought with silver, and it is not every one that can procure it. These wretched people first sold their wives, their sons and daughters, they then sold their tools, and the furniture of their houses; and even these they have finally demolished that they might sell the timber-work. Many of these unfortunate people were really rich four years ago."

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[*] This work is a collection of Chinese Natural History, compiled about A. D. 1575, from treatises which were still more ancient. M. S. Julien having kindly communicated to my son his copy of the *Pen-tsao-Kang-mou*, the quotation given in the Japanese Encyclopedia has been compared with the original text, and found to be accurate. Many of the places named are situated in the Northern Province called *Chan-Si*, where the cold is often severe during the winter; others belong to the maritime provinces of *Chan-tong* and *Kiang-Nan*, near the mouth of the Yellow River. The provinces of *Hou-Kouang* and *Kiang-Si*, concerning which the missionaries attest the same fact, are different from these, and are situated in the valley of the Blue River.

[†] See Annales de la propogation de la Foi, No. xlviii. p. 85, Sept. 1836.

Another missionary, M. Rameaux,* writing concerning the province of *Hou-Kouang*, about the middle of the year 1834, supplies details which are not less deplorable. "The district *Fan-Hien*, he remarks, contained about a thousand converts; but their number has been exceedingly reduced by famine. A great number have come to me to demand the last sacraments. They calculate their resources, and accurately know, almost to an hour, the number of days they can subsist. They receive the sacrament of extreme unction when their means are exhausted, and then having nothing to eat, they calmly wait the moment of their demise."

Clearly to apprehend the cause of these calamities, and their frequent returns among an industrious society, which is chiefly agricultural, and has had the blessing of a steady government for a long course of ages, it is necessary to recollect that many provinces of China, more extensive than the half of the whole kingdom of France, are great uniform plains, traversed by immense rivers, whose beds are ever and anon choked up by the deposits which are left by the waters, so that it is necessary constantly to confine them by high dikes, which are maintained with immense labour. The provinces of *Hou-Kouang* and of *Kiang-Si*, for example, which have now been named, are thus traversed by the Blue and other great rivers. These circumstances afford every facility for irrigation, develope an agriculture in which industry is pushed very nearly to its limit, whereby the most abundant harvests are produced, especially of rice, which is cultivated even up the slopes of the hills, the water being forced up by hand-engines. So long as this state of things continues, the necessary result is an immense production of the means of subsistence, which leads to a corresponding development of the population. But if once the waters so far increase as to run over the dikes, they spread over the plain, inundate it, and swallow up a portion of the population: whilst those who escape the disaster, finding themselves ruined, and deprived of all their resources so long as the waters cover the soil, remain a prey to all the miseries which the missionaries describe, and, finally, in immense numbers, actually perish from hunger. This cause, conjoined with the awful catastrophes produced by earthquakes, which seem to be more frequent, more violent, and especially more widely spread in China than in most other regions of the globe, enable us, in a great degree, to understand the sudden vicissitudes which, as the history of China attests, so often occur in the number of the population of this vast empire; vicissitudes whose proportionate number bears no relation to the regular laws of European population, as may be seen in a memoir inserted in the *Journal de la Société Asiatique*.†

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* Ann. de la propogation de la Foi, No. xlviii. p. 61.

† Memoire sur la population de la Chine et ses Variations, depuis l'an. 2400 avant l'ère Chritienne, jusqu'au 13e Siècle après; par Edouard Biot.

On the Colossal Fossil Mammiferous Quadruped, named Dinotherium Giganteum.

Fragments of the bones of this remarkable fossil and extinct species have been found in several parts of France, in Bavaria, and in Austria; the most abundant remains were found at Applesheim, in the province of Hesse Darmstadt, where an entire head, represented in Plate II., the most perfect specimen hitherto found, was discovered in the autumn of last year (1836.) The Applesheim bones were found in a sand pit along with *marine* shells, and those from France in a *fresh-water* tertiary limestone. It is described as having been one of the largest of the land mammalia, to have attained the length of eighteen feet, and according to Cuvier and Dr Buckland, as closely allied to the tapir. Dr Buckland in his Bridgewater treatise (vol. i. pp. 137–8) thus states his opinion respecting its habits:—

> "I shall confine my present remarks to this peculiarity in the position of the tusks, and endeavour to shew how far these organs illustrate the habits of the extinct animals in which they are found. It is mechanically impossible that a lower jaw, nearly four feet long, loaded with such heavy tusks at its extremity, could have been otherwise than cumbrous and inconvenient to a quadruped living on dry land. No such disadvantage would have attended this structure in a large animal destined to live in water; and the aquatic habits of the family of Tapirs, to which the Dinotherium was most nearly allied, render it probable, that, like them, it was an inhabitant of fresh-water lakes and rivers. To an animal of such habits the weight of the tusks sustained in water would have been no source of inconvenience; and, if we suppose them to have been employed as instruments for raking and grubbing up by the roots large aquatic vegetables from the bottom, they would, under such service, combine the mechanical powers of the pick-axe with those of the horse-harrow of modern husbandry. The weight of the head, placed above these downward tusks, would add to their efficiency for the service here supposed, as the power of the harrow is increased by being loaded with weights.
>
> The tusks of the Dinotherium may also have been applied with mechanical advantage to hook on the head of the animal to the bank, with the nostrils sustained above the water, so as to breathe securely during sleep, whilst the body remained floating at perfect ease beneath the surface; the animal might thus repose, moored to the margin of a lake or river, without the slightest muscular exertion, the weight of the head and body tending to fix and keep the tusks fast anchored in the substance of the bank; as the weight of the body of a sleeping bird keeps the claws clasped firmly around its perch. These tusks might have been further used, like those in the upper jaw of the Wal-

rus, to assist in dragging the body out of the water, and also as formidable instruments of defence.

The structure of the scapula, already noticed, seems to shew that the fore leg was adapted to co-operate with the tusks and teeth, in digging and separating large vegetables from the bottom. The great length attributed to the body would have been no way inconvenient to an animal living in the water, but attended with much mechanical disadvantage to so weighty a quadruped upon land. In all these characters of a gigantic, herbivorous, aquatic quadruped, we recognise adaptations to the lacustrine condition of the earth during that portion of the tertiary periods, to which the existence of these seemingly anomalous creatures appears to have been limited.

The head figured in Plate II. of the Dinotherium Giganteum has been carried to Paris, and submitted to the examination of French naturalists. At a meeting of the Academy of Sciences on the 26th March of the present year (1837), Mr Blainville read a communication detailing his views, in which he says:—

"The Dinotherium was an animal of the family of the Lamantins or Aquatic Gravigrades, its proper position being at the head of the family, preceding the Dugong, and consequently proceeded by the Tetracaulodon, which ought to terminate the family of the elephants. In a word, the animal, in our opinion, *was a Dugong with tusk-incisors.* We must then suppose that it had only one pair of anterior limbs, and five toes on each. As to the supposition that the animal was provided with a trunk, which might be presumed from the great nasal opening, the enlarged surfaces which surround it, and the size of the suborbital nerve, as far as it may be judged of from the size of the suborbital hole, we believe that is at least doubtful, and that it is more probable that these dispositions bear relation to a considerable development of the upper lip, and the necessary modification of the nostrils in an aquatic animal, as is equally the case in the Dugong."

More lately, M. Kaup, the discoverer of the Dinotherium Giganteum, who, in his work entitled "Das Theirreich," placed this fossil genus in the order *Edentata*, has reconsidered his former opinion; and in a letter in the Comptes Rendus for April 1837, proposes the following modification of it:—

"M. Kaup writes that the observations on the *Dinotherium*, which were presented by Messrs de Blainville, Isodore Geoffroy St Hilaire, and Duméril, at the meeting of the Academy of Sciences, on the 20th of March last, had led him afresh to examine the subject; and hence, thanks to the means of comparison supplied in the superb gallery of Comparative Anatomy, he had been enabled to satisfy himself that the alliances which he had first established between the animal and the *Edentata* were grounded upon deceptive appearances. I now recognise, says M. Kaup, that the two phalanges which I conceived I might refer to the *Dinotherium*, belonged to some other animal, which, without doubt, should be classed as a genus approaching to the *Pangolines* or *Orycteropes*; but in adopting upon this point, the opinion first advanced by Baron Cuvier, and which M. de Blainville has alluded to in his communication, I cannot see in the Dinotherium, as does this latter naturalist, an animal which very nearly approximates to the *Dugongs*. On the other hand, I think it must be placed among the Pachyderma properly so called, and in a genus very closely allied to the Hippopotamus. I shall state, in a few words, the reasons which induce me to think that the *Dinotherium* should not be arranged under the order Cetacea, but rather among the Pachyderma. 1st, The texture of the bones of the Cetacea differs completely from that of the Dinotherium; it is more fibrous, whilst in the bones of this animal it is harder, as in the pachyderma generally. 2d, The occipital bones of the cetacea present something like fontanells, which are especially remarkable in the neighbourhood of the basilar bone; whilst nothing of this sort is seen in the head of the *Dinotherium*. The *pars petrosa* of this last, which exhibits the same structure as in the pachyderma, is placed at the extremity of a long auditory canal, as in the hippopotamus, and consequently is not found situated on the level of the external face of the occipitals, as occurs in the dugongs, in which it forms a portion which is almost wholly isolated. 3d, As to the form, structure, number, and mode of replacement of the teeth, the *Dinotherium* is evidently one of the Pachyderma; and in this respect it has not the slightest analogy with the Manatee, and still less with the Dugongs. 4th, If the angle which the frontal bones forms with the back part of the cranium be excepted, there will be found in this last part, as M. Laurillard has remarked, much more resemblance with what we observe in the rhinoceros than with what is found in any other animal. But this obtuse angle, which I also find in the Cete properly so called, as I have formerly remarked, does not at all exist in the Dugong, in which this angle is almost a right one, as in the other Mammalia. 5th, The exterior form of the basilar bone, and of the bones which surround it, as well as that of the suborbital foramina, are entirely different from what is observed in the dugong, and exactly resembles that which is seen in the pachyderma. The same is also true of the prolongation in form of an epiphysis, which is found behind the glenoid cavity, or rather the facet which forms the articulation of the lower jaw; there is no analogy to this except in the pachyderma. 6th, The zygomatic arch, so far as we can judge of it by the portions of it which have been preserved, resemble those of the rhinoceros; in the dugong it is much more arched.—As to the cervical vertebra of the animal approximating the dugong, which is mentioned in the Catalogue of Fossiles of M. de Klipstein,—a vertebra which M. de Blainville alludes to as having possibly belonged to the *Dinotherium*, it belongs to an animal of the same size as the manatus, and consequently cannot have formed a part of the body of the *Dinotherium*; it belongs to a new genus, nearer to the manatus than the dugong, to which I have given the name of *Pugmeodon*; the animal is undoubtedly identical with that which has been described by M. Duvernoy, and the same also as the fossil manatus described by Baron Cuvier. The formation in which the bones of this animal is found is marine, and all the vertebræ, are filled with the teeth of the shark."

In Plate II. is a representation of the restored dinotherium as given by Kaup.

M. Strauss, a German naturalist, is not disposed to the opinions of Buckland or Kaup, but proposes one of the same general nature with that of Blainville. He writes in the following terms to the French Academy of Sciences,

"That he has at length formed an opinion concerning this animal which very nearly approaches to that delivered by M. de Blainville, but that he has been led to this conclusion by considerations which are altogether of a different character. It is not, he remarks, by investigating among the different animals, which are those whose heads most nearly approach to that of the *Dinotherium*, that I have been led to arrange this last among the Cete; but, on the other hand, by seeking to discover in the cranium the characters which it indicates; the arrangement which the other parts of the body must have assumed in regard to it, and the mode of life which this organization would necessarily require. In attributing to the *Dinotherium* a mode of life wholly aquatic, continues the author of the letter, I ground my opinion principally upon the shape and position of the occipital condyles, a position which proves that the series of the cervical vertebræ, and consequently that of the dorsal also, were in a horizontal direction; a position which could not occur in any of the terrestrial mammalia. In fact, according to a law which I shall have occasion to establish in a work upon the anatomy of the cat, which I hope will speedily be published, we are led to admit that in all the terrestrial mammalia the occipital condyles must be directed downwards for bipeds, and obliquely downwards and backwards for quadrupeds, so that the series of the cervical vertebræ, which is articulated with the condyles, may have the same direction, and so may contribute to support the head, and to arch it upwards and forwards, in its continuance with the series of the dorsal vertebræ. Now, in the *Dinotherium*, putting the plane of the molar teeth horizontal, the occipital condyles are directed obliquely backwards and upwards, a position which is altogether incompatible with a terrestrial mode of life, but perfectly possible in an aquatic animal, in which every part of the body, the head among the rest, is directly supported by the water.. Also, for this condition, it is likewise necessary that the cervical vertebræ should be directed backwards, as in truth we find them to be in the whales and in fishes. This first and principal character is, in addition, supported by the flattening of the occipital at its upper and back part, so furnishing the plan of the attachment of the extensive muscles of the head. This flattening has already been pointed out as a character which the *Dinotherium* possesses in common with whales, but not as indicating in itself an aquatic life. In fact the extensor muscles, by being fixed to this flattened portion of the head, would lose a great part of their power if the neck were directed downwards, the arm of the lever by which they act being thereby very much shortened. Thus, it is not because the *Dinotherium* exhibits in common with whales a flattening of the upper and back part of the cranium, that I judge that we must consider it a cetaceous animal, but because an aquatic life is a condition of this flattening in both the one and the other.

The disposition of the occipital condyles also proves that the *Dinotherium* was, not one of the amphibiæ, like the hippopotamus, the seals, and even the manatee, but an animal which, like the ordinary cetacea, can never come out of the water, except we at the same time admit very extraordinary conditions of organization, as if, for example, we were to suppose that the animal had the spinous processes of the cervical and dorsal vertebræ of an unheard of length, and so capable of giving attachment to enormous muscles which might freely support the head out of the water. The wholly aquatic mode of life of this singular animal being once admitted, it remains to determine what may have been its kind of nourishment. According to the form of its teeth, except the tusks, it is very probable that the *Dinotherium* was herbivorous; and this opinion is justified by the form of the glenoid cavity, which is entirely plane at its articulating part, as has been well remarked by M. Kaup, and not hollowed into a deep cavity, as stated by M. de Blainville. This proves that the jaw possessed a lateral motion, very advantageous in the grinding of its food. On account of the disposition of the occipital condyles, it was almost impossible for the animal to bend its head downwards, that so it might procure nourishment which was situated on the ground, an action which was moreover interdicted to it by the direction of its two tusks; it must, therefore, have seized its food in the same manner as the elephant, that is to say by means of a trunk. The existence of the trunk, which M. de Blainville regards as at least doubtful, is however indicated on the cranium by the great breadth and the disposition of the orifices of the nasal fossæ: lastly, it is also indicated by the disposition of the jaws, which* having no incisor teeth, wherewith directly to seize its food, cannot even meet at their anterior part, whilst they do so perfectly in the manatee. It might however be possible, though it is not at all probable, that the animal may have lived upon fish, which supposition is in no degree incompatible with the form of its teeth, although these differ very much from those of the dolphins, cete, which are essentially ichtyophagous; but this is one of those characters on which we ought not to found a great deal, no more than that we should place the *Dinotherium* in the genus tapir. Regarding the two tusks of the lower jaw, they appear to Messrs Kaup and de Blainville, to subserve the animal in turning up the soil, that it may thence procure the roots with which it was probably nourished. I do not participate in this opinion; these teeth rather appear to me to have contributed simply to its defence, as we find to be the case in elephants; for if these teeth had been employed in the manner these naturalists suppose, we should have found them in our specimen, much worn, whilst they are in a state of preservation which may be designated quite perfect. When the animal used them for the purpose I have alleged, it must have struck from above downwards, and to this end must have previously have much elevated its whole head, a circumstance which is likewise indicated by the disposition of the occipital condyles, which shews that the animal could easily raise its head till it made an angle of from fifty to sixty degrees with the horizon. In conclusion, as to the place the Dinotherium should occupy in the class Mammalia, I believe that it forms a distinct family among the Cetacea, and constitutes a link between the Pachyderma and the Cete. The hippopotamus amongst the Pachyderma would make the first step which would conduct to this new family, as the otter constitutes with regard to the Carnivora, the first step, which leads by the seals and the walrus to the manatees.

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* If there had been incisives in the upper jaw, they must have been very small.

Account of the Skull of a Fossil Quadrumanous Animal found in the Tertiary Rocks of the Sub-Himalayan Hills near the Sutlej.

In the 22d volume of this Journal, pp. 403–4–5, we gave an account of the jaw of a fossil monkey found imbedded in the tertiary formation of Simmore, in the department of Gers in France. We have now the pleasure of communicating a notice of the discovery in India of similar remains in tertiary strata by two active and intelligent engineers, Messrs W. E. Barker, and H. M. Durant. Their account is contained in the 59th number of that interesting periodical, the "Journal of the Asiatic Society of Bengal," from which the following is extracted.

"The specimen figured in Plate I. fig. 3, 4, was found in the hills near the Sutlej; and it appears from the attached matrix to have been derived from a stratum very similar in composition to the one described as occurring at the Maginund deposit. The fragment consists of the right half of an upper jaw; the molars, as to number, are complete, but the first has lost some of its exterior enamel, and the fifth has likewise had a portion of the enamel from its hind side chipped off. The second and third molars are a good deal worn; and the state of the fourth and fifth such as to indicate that the animal was perfectly adult. The canine is small, but much mutilated, its insertion into the jaw and its section being all that is distinct.

From the inspection of the molar teeth, the order to which the animal belonged is sufficiently evident; but there is enough of the orbit remaining to afford additional and very satisfactory proof; the lower part of the orbit and the start of the zygomatic arch being very distinct, would alone remove all doubt from the subject, the orbits of the Quadrumana being peculiar, and not easily to be confounded with those of other animals.

On comparison with the delineations of the dentition of this order of animals given by F. Cuvier, the fossil bears some resemblance to the genus *Semnopithecus*; the section of the canine, and the form and size of the false molars, are very similar to the exemplar taken by F. Cuvier from a head of the species *Maurus*, a species found in Java; had the drawing been taken from the *Entellus*, a species which inhabits India, the comparison would in this instance have been more satisfactory; the *Maurus* being chosen as the type, and no mention made of other difference except length of canines, the various species may be supposed to present no material departure from the type in form of molars. The third molar in the fossil is so much worn as not to admit of being compared with drawings from unworn teeth; the fourth is like that of the *Maurus*; but the fifth does not resemble the analogous molars of any of the existing species, as represented by F. Cuvier, for the fossil tooth possesses a small interstitial point of enamel at the inner side, which does not appear to have place in any of those delineated. The incisors are absent; but the intermaxillary is clearly distinguishable.

Were it not for the size of the canine and fifth molar, the specimen presents some resemblance to the genus *Macacus*, given as the type of the genera *Macacus* and *Cynocephalus*; the smallness of the canine and the larger size of the molars causes the fossil to approach more nearly to the *Semnopithecus* than to the *Macacus*; the difference is, however, great between the two, for the *Entellus* is said to attain the length of three and a half feet, whereas the length of the fossil animal, if the space occupied by the molars and their size be deemed sufficient ground for a conjecture, must have been equal to that of the *Pitheeus Satyrus*,—the space taken up by the molars is 2.15 inches. This circumstance, and the differences before pointed out, clearly separate the fossil from the species belonging to the genera *Cynocephalus Semnopithecus*. The specimen is imperfect, but it indicates the existence of a gigantic species of Quadrumanous animals contemporaneously with the Pachyderma of the Sub-Himalayas, and thus supplies what has hitherto been a desideratum in Palæontology,—proof of the existence in a fossil state of the type of organization most nearly resembling that of man." Fig 3. in Plate I. is a little fore-shortened, in order to shew the bottom of the orbit at the hollow in the upper part of the skull. (Both figures were taken with the camera lucida.)

SCIENTIFIC INTELLIGENCE.

CHEMISTRY.

1. *On the Burning of Limestone, or the Decomposition of Carbonate of Lime by Heat.*—M. Gay-Lussac observes, that it has long been supposed that the calcination of limestone is accelerated by the presence of water; and the opinion appears to be adopted by lime-burners in general. M. Dumas admits the influence of water to be unquestionable, and he gives two explanations of its action; either, says he, it acts upon the carbonate, and forms a temporary hydrate, taking the place of the carbonic acid for a very short time, for the hydrate of lime itself is decomposed by a red heat; or the water being decomposed by the carbon, employed as a combustible, is converted into various gases, of which carburetted hydrogen forms a part, and this reacting upon the carbonic acid of the carbonate, tends to convert it into oxide of carbon, and thus facilitates the separation from the carbonate of lime. Thus, limestone fresh quarried, and consequently still moist, ought to be more readily calcined than the

stone which is nearly dry; and most lime-burners are well acquainted with this fact, and sprinkle with water the limestone which has been long procured before they charge the kilns with it."—(Dumas, *Traité de Chimie*, ii. p. 482.) The first of these explanations is, however, inadmissible, for hydrate of lime is decomposed at a temperature lower than that at which carbonate of lime is decomposed under the influence of water. On considering the circumstances of the combustion in limekilns, the second explanation does not appear to M. Gay Lussac to be applicable, and he therefore proceeds to some observations which he thinks will explain the influence of the water. A porcelain tube was filled with bits of marble and placed in a furnace, the heat of which was easily regulated; a glass retort containing water was adapted to one end of the tube, and at the other a glass tube to receive the carbonic acid gas. The heat was raised sufficiently high to decompose the marble, and on shutting the ash-pit door the heat fell to a dull red, and the carbonic acid ceased to come over; and at this instant the water was boiled in the retort, and carbonic acid was abundantly obtained. On discontinuing the vapour, the disengagement of acid instantly ceased, and returned only on continuing the vapour. It appears, therefore, proved that the vapour of water actually favours the decomposition of the carbonate of lime by heat, and that by its operation this decomposition may occur at a lower temperature than is otherwise requisite. M. Gay-Lussac considers the action of the water to be entirely mechanical. When the carbonate of lime is exposed to heat, and is near the point of decomposition, an atmosphere of carbonic acid is formed around it, which presses upon the acid remaining combined, so that the latter, that it may be disengaged, must overcome the pressure of this atmosphere. This, however, cannot occur without still further raising the temperature, or removing the carbonic acid and forming a vacum, or by displacing it, either by the vapour of water, or some other elastic fluid, such as atmospheric air. This explanation is supported by the following experiment: Carbonate of lime was heated in a porcelain tube nearly to its decomposing point, and hen a current of atmospheric air was passed over it. The dis- gagement of carbonic acid immediately commenced, continued ith the current of air, ceased when it was stopped, and recommenced with it. M. Gay-Lussac, therefore, considers it as proved, that the influence of aqueous vapour, in the calcination of limestone, is confined to the production of a vacuum for carbonic acid, and to the prevention of the pressure of the disengaged acid, upon that which remains with the lime. When the vapour is present, a lower temperature is required to dislodge the carbonic acid; but the importance of this influence must not be overrated. The water, in calcareous stones, is mechanically interposed between their particles; and, with the exception of some minute portions, which remain confined in the centre of pieces too large to allow of the heat penetrating and vaporizing them, the greater part of the water must evaporate without any useful result; and, on the contrary, with the loss of fuel, before the limestone has acquired the temperature requisite for its decomposition. M. Gay-Lussac thus concludes:—"I am certainly convinced that the vapour of water favours the calcination of limestone, but I am doubtful as to its possessing real advantages, because there is not a great difference in the temperature at which it decomposes alone or with the vapour of water; besides, if the vapour of water only exerts a mechanical action similar to that of atmospheric air, any important advantage which it possesses over the aëriform current continually passing over the burning mass, is not evident. The readier decomposition of carbonate of lime by the access of aqueous vapour, or rather by means of a vacuum, cannot be considered as an isolated fact. It may be regarded as an established principle, that when one or several gaseous products are obtained, either by the action of heat or a chemical agent, the decomposition may be generally facilitated by keeping the bodies in vacuo, or by preventing the gaseous fluids from pressing upon it. And on the other hand, the decomposition may be retarded or entirely prevented, by forming round the body a sufficient pressure of an elastic fluid of the same nature as that which is to be disengaged. It is thus in the curious experiment of Sir James Hall; carbonate of lime was fused at a very high temperature, without being decomposed, under the influence of a sufficient pressure of carbonic acid.—*Ann. de Ch. et de Ph.* lxiii. 219.

METEOROLOGY.

2. *St Elmo's Fire seen in Orkney.*—During last February, 1837, (Sunday 19,) in a tremendous gale my large boat sunk, and it was late on Tuesday night before we could get her up and drawn to the shore, after which we had to wait till three o'clock next morning till the tide ebbed from her; she was during this time attached to the shore by an iron chain, about 30 fathoms long, which did not touch the water, when to my astonishment I beheld a sheet of blood-red flame, extending along the shore for about 30 fathoms broad and 100 fathoms long, commencing at the chain and stretching along the shore and sea in the direction of the shore which was E. S.E., the wind being N. N.W. at the time. The flame remained about ten seconds, and occurred four times in about two minutes. Whilst I was wondering not a little, the boatmen, who, to the number of twenty-five or thirty, were sheltering themselves from the weather, came running down apparently alarmed, and asked me if I had ever seen anything like this before. I was about to reply, when I observed their eyes directed upwards, and found they were attracted by a most splendid appearance at the boat. The whole mast was illuminated, and from the iron spike at the summit, a flame of one foot long was pointed to the N. N.W., from which a thunder-cloud was rapidly coming. The cloud approached, which was accompanied by thunder and hail; the flame increased and followed the course of the cloud till it was immediately above, when it arrived at the length of nearly three feet, after which it rapidly diminished, still pointing to the cloud, as it was borne rapidly on to S. S.E. The whole lasted about four minutes, and had a most splendid appearance. I regretted afterwards that I was so occupied with the flame at the mast head that I did not observe whether the red flame on the ground continued during the time the cloud was passing.—*Extract of a Letter from William Traill, Esq., Kirkwall, to Professor Traill, dated May* 16. 1837.

MICROGRAPHY.

4. *Microscopic Differences in Cotton, Lint, and Hemp.*—M. Dutrochet lately read to *L'Academie des Sciences* a notice concerning the vegetable matter which had been employed in the fabrication of the cloths which envelop the Egyptian mummies. Hitherto

it has usually been supposed that these cloths were composed of cotton; but lately Mr James Thomson and Mr Bauer have published in this country the results of their researches, which go to shew that it was lint and not cotton which was employed in their construction. It was by the means of the microscope, and by comparing the form of the filaments of cotton and lint, that these gentlemen come to this conclusion. The filaments of the former are flat and twisted upon themselves, and resemble small ribbons twisted in such a way as to represent a long flat plate spirally arranged; on the contrary, the filaments of lint are cylindrical throughout. The form of the filaments of cotton may be discovered both in the threads of cloth, and also in paper which has been made from cotton rags. But nothing resembling this form has been observed in the filaments of the thread which compose the cloths round the Egyptian mummies; and, on the other hand, the cylindrical form of the filaments of lint is discovered. M. Dutrochet has repeated the observations of Messrs Thomson and Bauer upon the enveloping wrappers of the mummies contained in the Egyptian Museum at Paris; and has likewise found that the threads of these cloths entirely resemble those of linen, and are quite different from those of cotton. He has likewise remarked another circumstance which has not been noted by the above named gentlemen. In examining the woven filaments of lint taken from threads which have so long been subjected to use, that the frequent washings have completely destroyed the natural adherence of the different filaments, an adhesion which usually is not destroyed by the steeping, he has noticed that these filaments are of two kinds; the one, like microscopic bamboos, are vegetable tubes composed of elongated joints, and often somewhat swollen at the knots which are formed by their junction; these tubes are about the .0004 of an inch in diameter. The other vegetable tubes which, with the preceding, constitute the textile filaments of lint, are not composed of joints, but are uniform in their appearance, and their diameter is about half the size of the others. It is to be observed in addition then, that these two kinds of textile filaments which have thus been found by M. Dutrochet in the lint of our day, have been also observed in the threads which were employed in the fabrication of the cloths which surround the Egyptian mummies.

That he might ascertain whether any of the cloths of ancient Egypt were made of hemp, M. Dutrochet has examined with the microscope the weavable filaments of this last vegetable. These last filaments, like those of lint, are of two kinds, the one jointed and the other uniform and continuous. In general the textile filaments of hemp are larger than those of lint; the jointed filaments are about .0008 of an inch in diameter, that is to say, about twice the size of the analogous ones of lint. With this information, M. Dutrochet has come to the conclusion that none of the ancient tissues obtained from Egypt which he has examined, have been made of hemp. Hence it is lint alone which has been used by the ancient Egyptians in the fabrication of such of their cloths as were manufactured from vegetable matter, and we may thence conclude that, contrary to the general opinion, they were not acquainted with cotton. The question then occurs, what is the substance which is called *byssus* (βύσσος) by Herodotus, and with which, according to him, was manufactured the cloth with which the mummies were enveloped. "May we not conceive," says M. Dutrochet, "that this word may have expressed the filamentous weavable matter which lint supplied, in the same way as the words *flax* and *tow* express among ourselves that same filamentous weavable material, supplied both by lint and hemp. This will explain the cause of the error so generally committed by the learned, who, perceiving in Herodotus that the cloths of the ancient Egyptians were manufactured from lint and *byssus*, thence concluded that the lint was different from the *byssus*. Having once come to this conclusion, they inferred that byssus could be nothing else than cotton."—M. Dutrochet concludes by telling us that the twisting of the Egyptian threads was effected in a direction contrary to the one now generally in use. After the reading of this communication, M. Costaz remarked, that, amongst the paintings of the grottos of Elethyia, and whose description is given in the great work on Egypt, a field is represented in which the workmen are engaged in pulling up lint, and separating the seed from the stalks. This observation, which proves the culture of lint on a great scale in ancient Egypt, suggested to M. Costaz reflections respecting the wrappings of the mummies analogous to those which had occurred to M. Dutrochet.

List of Patents granted in Scotland from 14*th March* 1837 *to* 12*th June* 1837.

1. To Thomas Theophilus Biggs, of Queen Ann Street, Cavendish Square, in the county of Middlesex, Esquire, in consequence of a communication made to him by a certain foreigner, and invention by himself, of "improvements in certain descriptions of fire-arms."—Sealed 17th March 1837.

2. To John Leberecht Steinhaeuser, of Upper Terrace, Islington, in the county of Middlesex, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in hand and power looms."—Sealed 17th March 1837.

3. To Fletcher Woolley, of York Street, East Commercial Road, in the county of Middlesex, gentleman, for an invention of "improvements in the manufacture or preparation of materials to be used as a substitute for bees' wax, parts of which improvements are applicable to other purposes.—Sealed 17th March 1837.

4. To Neil Snodgrass, of the city of Glasgow, in the county of Lanark, engineer, for an invention of "improvements in steam-engines, and other mechanism of steam-boats," which were partly communicated by a foreigner residing abroad, and partly invented by himself.—Sealed 21st March 1837.

5. To Miles Berry, of the office for Patents, Chancery Lane, in the county of Middlesex, patent agent and mechanical draftsman, in consequence of a communication from a foreigner residing abroad, for an invention of "certain improvements in cleaning, purifying, and drying wheat and other grain or seeds."—Sealed 22d March 1837.

6. To Samuel Tonkin Jones, of Manchester, in the county palatine of Lancaster, merchant, for an invention of "certain improvements in the tanning of hides and skins."—Sealed 29th March 1837.

7. To Charles Brandt, of Upper Belgrave Place, in the county of Middlesex, mechanist, for an invention of "an improved method of evaporating and cooling fluids."—Sealed 31st March 1837.

8. To Charles Pierre Devaux, of Fenchurch Street, in the city of London, merchant, in consequence of a communication from a foreigner residing abroad, for an invention of "a new or improved apparatus for preventing the explosion of boilers or generators of steam."—Sealed 7th April 1837.

9. To William Hancock, of Windsor Place, City Road, in the county of Middlesex, gentleman, for an invention of "certain improvements in bookbinding."—Sealed 8th April 1837.

10. To Richard Burch, of Heywood, in the county of Lancaster, mechanist, for an invention of "certain improvements in locomotive steam-engines, to be used either upon rail or other roads, which improvements are also applicable to marine and stationary steam-engines."—Sealed 14th April 1837.

11. To Henry Backhouse, of Walmsley, in the parish of Bury, calico-printer, and Jeremiah Grime, of Bury, both in the county of Lancaster, engraver, for an invention of "certain improvements in the art of block-printing."—Sealed 14th April 1837.

12. To William Nairne, flax-spinner, Millhaugh, near Methven, in the county of Perth, for an invention of "a certain improvement, or certain improvements, in the machinery of reels used in reeling yarns."—Sealed 14th April 1837.

13. To Bennet Woodcroft, late of Ardwick, in the parish of Manchester, in the county of Lancaster, but now of Mumps, in the township of Oldham, in the same county, gentleman, for an invention of "an improved mode of printing certain colours on calico and other fabrics."—Sealed 18th April 1837.

14. To George Crane, of Yniscedywyn Iron Works, near Swansea, ironmaster, for an invention of "an improvement in the manufacture of iron."—Sealed 26th April 1837.

15. To Nathaniel Partridge, of Elm Cottage, near Stroud, in the county of Gloucester, gentleman, for an invention of "a certain improvement, or certain improvements, in mixing and preparing oil paints, whereby a saving of ingredients commonly used will be effected."—Sealed 27th April 1837.

16. To James Hardy, of Wednesbury, in the county of Stafford, gentleman, for an invention of "certain improvements in the manufacture of iron into cylindrical, conical, and other forms, suited for axletrees, shafts, and other purposes."—Sealed 27th April 1837.

17. To Christopher Nickels, of Guilford Street, Lambeth, in the county of Surrey, gentleman, partly in consequence of a communication made to him by a certain foreigner resident abroad, and partly by his own invention, for "improvements in preparing and manufacturing caoutchouc, applicable to various purposes."—Sealed 29th April 1837.

18. To William Coles, of Charing Cross, in the county of Middlesex, Esquire, for an invention of "certain improvements applicable to locomotive carriages."—Sealed 29th April 1837.

19. To Moses Poole, of the Patent-office, Lincoln's Inn, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in making fermented liquors."—Sealed 10th May 1837.

20. To Joseph Bunnett, of Newington Causeway, in the borough of Southwark, window-blind-maker, for an invention of "certain improvements in window-shutters, which improvements may also be applied to other useful purposes."—Sealed 12th May 1837.

21. To John Samuel Dawes, of Birmingham, in the county of Warwick, iron-master, for an invention of "certain improvements in smelting the ores or oxides of iron, copper, tin, lead, zinc, and other metals, and in remelting or refining the said metals."—Sealed 15th May 1837.

22. To Joseph Amesbury, of Burton Crescent, in the parish of St Pancras, and county of Middlesex, surgeon, for an invention of "certain apparatus for the relief or correction of stiffness, weakness, or distortion, in the human spine, chest, or limbs."—Sealed 20th May 1837.

23. To John Gordon Campbell, of the city of Glasgow, in the county of Lanark, merchant, and John Gibson, of the same city and county, throwster, for an invention of "a new or improved process or manufacture of silk, and silk in combination with certain other fibrous substances."—Sealed 20th May 1837.

24. To Henry William Crafurd, of John Street, Berkeley Square, in the county of Middlesex, Commander in the Royal Navy, for an invention of "an improvement in the coating or covering iron and copper for the prevention of oxidation."—Sealed 22d May 1837.

25. To Charles Guynemer, of Manchester Street, Manchester Square, in the county of Middlesex, professor of singing, for an invention of "certain improvements in piano fortes," communicated to him by a foreigner residing abroad."—Sealed 24th May 1837.

26. To William Bridges Adams, of Porchester Terrace, Bayswater, in the county of Middlesex, coach-maker, for an invention of "certain improvements in the construction of wheels, and in wheel carriages."—Sealed 2d June 1837.

27. To William Gossage, of Stoke Prior, in the county of Worcester, chemist, for an invention of "certain improved apparatus for decomposing common salt, and for condensing and making use of the gaseous product of such decomposition; also certain improvements in the mode of conducting these processes."—Sealed 2d June 1837.

28. To John Joseph Charles Sheridan, of Ironmonger Lane, of the y of London, in the county of Middlesex, chemist, for an invention of ertain improvements in the several processes of saccharine, vinous, and tous, fermentation."—Sealed 6th June 1837.

9. To Pierre Bartlemy Guinibert Debae, of Brixton, in the county Surrey, civil-engineer, for an invention of "improvements applicable to ail-roads."—Sealed 12th June 1837.

SOCIETY OF ARTS.

ALPHABETS FOR THE USE OF THE BLIND.

[The present Number of the Journal was completed before this notice was received. We have therefore inserted it here, although rather out of place.]

At the concluding meeting of the Session of this Society, held on the 21st instant, the Committee which had been appointed to consider the numerous Alphabets for the Blind (a plate of which was appended to the 42d Number of this Journal for October 1836), and relative communications, which had been received in competition for the Society's Gold Medal, value Twenty Sovereigns, reported, that they had, after nearly four years' investigation, brought their labours to a close. From the Committee's very long and detailed report, consisting of upwards of thirty closely printed pages, it appeared that they had corresponded with all the institutions for the Blind which could be ascertained to exist in Great Britain and Ireland,—with the British Association, which had lately taken up the same subject,—and with such philanthropic individuals both in Britain and abroad as were known to take an interest in the education of the Blind. The result of the whole was, that the Committee were of opinion,

"1. That although an arbitrary character might possess in itself superior advantages in simplicity and tangibility, yet there would be great and in many cases insuperable obstacles to the Blind generally acquiring a knowledge of any character not familiar to those possessed of sight, and consequently such an alphabet would not be generally adopted throughout Europe and America.

"2. That the same objection applies, although perhaps in a less degree, to Mr Gall's angular modification of the Roman alphabet; and, while the want of capitals and the difficulty of tracing the lines are said to be also serious objections to the use of his character, it does not in other respects seem to offer sufficient reasons for its adoption, in preference to the ordinary Roman alphabet slightly modified.

"3. That, from being almost universally known both in Europe and America, and taking all other circumstances into consideration, the common Roman capital alphabet, as represented by the late Dr Fry, seems not only to be best adapted for teaching the Blind to read, but also as a medium of written correspondence. Hence there is every reason to believe, that it would be sooner brought into general use than any of the other characters in competition—that books printed with it would be more in demand—and, consequently, that their expense would be greatly diminished." * * * *

"Upon the whole—looking to the terms of the Society's advertisement, and the other circumstances above referred to—the Committee beg to state, that, in their opinion, the late Dr Fry's communication is entitled to 'The Society's Gold Medal, value Twenty Sovereigns,' being the prize offered 'for the best communication on a method of Printing for the Blind,' &c." * * * * *

"It may be here also proper to notice, that in proposing the late Dr Fry's communication as best entitled to the Society's Premium, the Committee do not wish it to be understood that they consider his modification of the Roman Alphabet as *now* in every respect the best adapted for teaching the Blind; but only that it was superior to any of the others given in to the Society for competition, and remitted to the Committee for consideration. Several material improvements on this alphabet have been since proposed and partly carried into effect, by the Rev. Mr Taylor, Mr Anderson of York, and Mr Alston of Glasgow; and the Committee would also suggest as a farther and very important improvement, both as respects economy in printing and facility in reading, the adoption of the *fretted surface* of type recently introduced by Mr Gall on his angular character, *and likewise* his method of printing on both sides of the paper."

With reference to these latter improvements, and a recent relative communication from Mr Gall jun., the Committee also reported, that—

"In ordinary books, the letter-press is more easily read by those who see, when the lines are a little separated from each other, than when they are set close together; and much more so must this be the case with books for the use of the Blind. The difficulty experienced by the Blind in reading a closely printed page, has been ingeniously obviated by Mr Gall, by simply leaving blank spaces between each printed line, sufficiently large to admit of a corresponding number of lines being printed in these spaces on the reverse sides of the page. Each leaf of paper is thus printed on both sides, like books for the seeing, while to the *feel* it appears to be only printed on one side; and the lines are at the same time perfectly distinct and separated, so as to prevent the finger running from one line into another—an objection said to be hitherto specially applicable to Mr Gall's angular character of type.

"The plan of *fretting*, or giving the types a dotted or rough surface, as hinted at in some of the earlier communications to the Society, must also very considerably facilitate the processes both of printing and reading. By this means, the severe labour of press-printing for the Blind is much diminished, and the fretted surface is well adapted for facilitating the progress of reading by the sense of touch.

"On the whole, from these and other recent improvements in the means of educating the Blind, the Committee are hopeful that such a simple modification, and judicious combination, of both capitals and small letters, from the various alphabets now before the Society, will be speedily accomplished by Mr Gall, Mr Alston, or some of the other philanthropic gentlemen now practically engaged in this important matter, as will give general satisfaction, and be universally adopted for the use of the Blind."

The Society unanimously approved of both of these Reports, and ordered them to be printed for circulation in their Transactions. Mr Gall junior has since sent to the Secretary the following communication:—

"24. *Niddry Street*, 21*st June* 1837.

Dear Sir,—In consequence of the adoption of the report upon the Alphabets and methods of printing for the Blind, which were submitted for competition some years ago, in which the capitals proposed by the late Dr Fry are preferred, and as the Society has also recommended their adoption along with the methods invented by my father and myself of printing by means of the fretted types, and on both sides of the paper, I beg to state that my father, in compliance with their recommendation, will unite the two objects, and print in the manner which they recommend.

I beg also to state, that neither my father nor myself will consent to any individual making use of the inventions of fretted types and printing on both sides of the paper; for although the expense of a patent prevented our securing *by law* a property in these inventions, yet we appeal to every honourable mind whether, when our wish is thus expressed, any one ought to take advantage of this circumstance.

If receiving any premium from the Society for these inventions be considered a permission for any one to make use of them, we beg most respéctfully to decline it.

I would feel much obliged by your mentioning this in any publication or letter in which the decision of the Society is announced.

I am, Sir, your most obedient Servant,

James Gall, Jun.

James Tod, Esq. Sec. Soc. Arts, &c."

The other Candidates, besides Dr Fry, particularly noticed in the principal Report, are Messrs Hay, Gall, Ponton, Lothian, Milne, and Richardson, all of whom had evidently devoted great attention to the subject; and the Committee expressed their obligations in an especial manner to the Rev. Mr Taylor, Dr Carpenter of Bristol, and Mr Alston, for their valuable assistance throughout the inquiry.

The following were the Members of Committee:—

Sir Thomas Dick Lauder, Bt., late V.P. S.A.
Patrick Neill, Esq. LL.D., F.R.S.E.
John Robison, Esq. Sec. R.S.E.
George Mackillop, Esq.
John Dunn, Esq., Cur. S.A.
Rev. Edward Craig, A.M. Oxon.
William Beilby, Esq., M.D.
Robert Bald, Esq., F.R.S.E., V.P. S.A.
John S. More, Esq., F.R.S.E., Couns. S.A.
Edward Sang, Esq., F.R.S.E., Couns. S.A.
William Fraser, Esq., Couns. S.A., *Convener*.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Biographical Memoir of EDWARD TURNER, *M. D., F. R. S. L. & E. Professor of Chemistry in the University of London, &c.* By ROBERT CHRISTISON, M. D., F. R. S. E. Professor of Materia Medica in the University of Edinburgh, &c.*

GENTLEMEN,—THE subject I propose to bring before you on the occasion of our again meeting together, is a biographical sketch of the late Professor of Chemistry in University College, London, our departed friend, Dr Edward Turner.

I am sensible that my choice of a subject is in one respect unusual; for Dr Turner never was our fellow member. But I feel confident that more than an apology will be found by you all in his fame as a man of science, his reputation as a teacher his worth as a man,—in his recent death, while in the vigour of his age, and in the ties of friendship which bound him to many now before me. There are passages in his life, which, for the sake of the rising generation, ought not to be lost, and some particulars, too, from which all may take a useful lesson. If, on these accounts, a biographical tribute be due to his memory, on no one can the task of rendering it be so justly charged as upon myself, the constant companion of his youth, the most intimate friend of his manhood; and I am sure I should nowhere meet with so many willing and affectionate listeners as in this Society, where all can well appreciate his scientific excellence, and not a few recall him as a beloved member of their social circle.

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* The above Memoir was read before the Harveian Society of Edinburgh on the 12th of April 1837, and printed at the request of the Society.

Our colonial settlements, vastly as they have contributed to the materials of science, have added but few to its votaries. The subject of the present memoir is a rare exception. Dr Turner was born in Jamaica in July 1796. His father, Mr Dutton Smith Turner, an Englishman by birth, was a prosperous planter in that island; and his mother, Mary Gale Redwar, was a Creole of English parentage. Edward was the second son of a numerous family, of whom seven are still living. In early childhood his parents brought him to Britain, which he never subsequently left till his education was finished.* For many years Dr Turner resided in Bath or its neighbourhood, and he received the rudiments of knowledge first at a lady's school, and afterwards at the grammar-school of that city, then conducted by the Reverend Nathaniel Morgan. During this period, which extended till his fifteenth year, he seems to have shown no remarkable talent, and to have given no promise whatever of future assiduity or eminence. He resided constantly with a relative of his mother, a country gentleman of high principle and honourable feeling; who paid great attention to his moral character and conduct, but who, being himself ardently devoted to the pursuits of the field, and sports of the country, could not be expected to set his ward the example of study, or of the love of learning. It is not uninteresting to trace the total want of connexion between his early habits, acquired at this time under his guardian and teachers in Bath, and the course into which he was gradually turned in after life by his own unaided judgment and efforts. When he left the grammar-school, he was remarkable for nothing but a mild, engaging, affectionate disposition, a very small amount of learning for his years, and a great fondness for angling, and other country sports and exercises. Ten years later, he was distinguished among those of his standing for professional knowledge and untiring application; of his earlier habits he retained only a love of country rambling; and as an angler, I can bear testimony to his having then possessed equally little skill and enthusiasm.

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* His parents returned soon after to Jamaica, and remained there till Mr Turner's death in 1816.

At the age of fifteen, having made choice of medicine for his profession, he was apprenticed with a country practitioner not far from Bath. After remaining here for three years, at a period usually the most important in the course of a young man's education, he left his master's roof with a fund of general knowledge, and an amount of acquaintance with literature and science, very much the same with what he possessed on entering it. I have his own authority for the fact, that, when he commenced his apprenticeship, he could scarcely solve a question in the rule of three; and he has often spoken to me of his three years' servitude as a blank space of time, unmarked by the memory of a single useful acquisition. His case in this respect is far from being a singular one. Many of his contemporaries and successors could tell a similar tale; and, assuredly, of all the varieties of a system of education, at the very best equivocal in its results, a country apprenticeship between the ages of fifteen and eighteen seems about the least calculated to supply either the means of information or the incentives to use them.

Quitting at length this scene of unprofitable occupation, Dr Turner went to London in 1814, where he prosecuted his studies for two years as a house-pupil of Mr Andrews, one of the surgeons of the London Hospital. For the advice and instructions of this gentleman he often in after-life expressed his gratitude and respect; and he used to consider his time during this period as having been usefully spent. If such really was the case, it illustrates well the defective state of his earlier education and habits; since even in London he was not at all distinguished among his fellow-pupils, and did not betake himself with vigour to the study of the medical sciences.

In November 1816, being now in his twentieth year, Dr Turner repaired to the University of Edinburgh with the view of graduating; and after passing through the usual curriculum of study, at that time limited to three years, he received his degree in August 1819. It was about the middle of that term that my acquaintance with him commenced. He was then, as ever afterwards, in person tall, slender, but sinewy; of easy and engaging address, fluent and animated in conversation, and presenting throughout all his intercourse with his fellow-students, the happiest combination I have ever known of urbanity and mildness with high spirit and independence. His character in this respect was well developed at the meetings of the Medical Society, of which he was in the Session of 1818–19 one of the annual Presidents. On this often stormy arena, he was a spirited and impressive debater, and also a universal favourite, by reason of his never-failing good humour and conciliating disposition. That he was a President of this admirable institution is of itself sufficient proof that he had now become a diligent and successful student, a part of his character for which there is reason to think, he was largely indebted to the stimulus of ambition, first fairly kindled within him by what he witnessed at the Society's meetings in the earlier period of his membership.

As a student, his attention was mainly turned to practical medicine; and with a view to a more exact cultivation of this department, he first became a clinical clerk under the present Dr Home, at that time the most popular of the clinical professors, and he subsequently obtained the appointment of resident clerk in the fever hospital. In this institution, recently founded on account of the appearance of a wide-spreading epidemic, the same which visited almost every great town in the British Empire during the years 1817–18–19 and 20, Dr Turner had ample opportunities of practical instruction; for the particular variety of fever then prevalent, presented features of unusual interest, and the comparative absence of fever in the epidemic form for many years before, led to its being studied with unusual assiduity by all practitioners in this city. Dr Turner's share in observing and treating it was ample; and he has left sufficient evidence of the interest he took in this pursuit; for the prevailing epidemic formed the subject both of the paper which he was required in rotation to present to the Medical Society, and also of his thesis delivered on the occasion of his graduation. The subject of the former was—"On the Continued Fever which was epidemic in Great Britain and Ireland during the years 1817–18 and 19, exemplified by a description of that which prevailed in Edinburgh." The title of the latter is, "De Causis Febris

Epidemicæ nunc Edinburgi grassantis." His thesis was one of the best in a year which produced an unusual number of great merit. It clearly bears the impress of those qualities which subsequently distinguished him as an author and a teacher,—an attachment to facts over theories, unwearied assiduity as an observer, for his statements are founded entirely on what he himself witnessed and collected, a habit of close observation and of cautious induction, together with uncommon ease, precision, and perspicuity in explaining his views. In this treatise he warmly espouses the doctrine of the contagious origin of fever; and he shews that the practice of medicine was at the time, as indeed I know it really was, the undivided object of his pursuits.

It is not a little remarkable, that hitherto Dr Turner had shewn no leaning towards that branch of science of which he was subsequently to become so ardent and successful a cultivator. Of chemistry he possessed the ordinary knowledge acquired by the better class of Edinburgh graduates, at a time when it was not taught practically as now. But he possessed no more. He had no practical acquaintance with it, no fondness for chemical research. In short, none of his intimates ever dreamed that he should one day become a chemist. Indeed, two years more elapsed before his views peculiarly turned in that direction; and it may be safely said, that at the age of twenty-five, he had forgotten the little chemical information he had once acquired; so that his chemistry was probably much in the same case with his arithmetic ten years earlier.

It had been long intended by his friends, that he should take advantage of an offer made many years before by an eminent practitioner in Jamaica, of receiving him as a member of a prosperous medical copartnery in Spanish Town. But this arrangement did not eventually suit his inclinations; the offer was transferred to his younger brother; and he himself determined to try his fortune as a physician in Bath, whither he accordingly returned in the winter of 1819.

Either, however, the slow approach of professional business, or more probably, the consequent want of adequate and definite exercise for his mind, now awakened to full activity, speedily disgusted him with his prospects; and after going through the forms of settling as a physician, he suddenly, in the summer of

1820, altered his determination, and resolved to visit the Continent; in the first instance, I suspect, with no very precise views beyond present useful occupation. Accordingly, in August of that year, he resorted to Paris in company with the late Dr William Cullen and myself, and remained there for nine months, frequenting the hospitals, studying the French and German languages, and commencing also to supply the defects of his early education, by cultivating physical science and modern history. Still, however, chemistry formed no prominent part of his studies. But it was while he resided in Paris that he determined on making this branch of science his fundamental pursuit.

It does not exactly appear what led him to make so abrupt and extraordinary a change in his professional objects. There are certainly few courses of preparatory training which seem less fitted to secure proficiency in chemical science, or less likely to instill a fondness for it, than the cultivation of pure pathology and therapeutics. Yet such had hitherto been the chief object of Dr Turner's education, and such the only branches of science for which he had hitherto shown an attachment. That he should all at once have resolved at the age of twenty-five on exchanging these pursuits for one, which could not be successfully followed in its improved modern form without at least three qualifications of which he was at the time almost utterly destitute, namely a knowledge of mathematics, a knowledge of various branches of physics, and an intimate acquaintance with apparatus and the art of manipulating,—does certainly seem not a little singular. That he should remedy all these defects, conquer every obstacle, make himself in a few years thoroughly and practically acquainted with every important department of so varied a science, and acquire above all a facility and exactitude as an experimentalist which could scarcely be surpassed,—is one of the instructive events in his history over which the mental philosopher may usefully ponder, and from which the youthful and aspiring mind may draw most wholesome advice and encouragement.

During the period of his residence in Paris his attention appears to have been keenly turned to experimental science in general, in consequence of the extraordinary interest excited at the time by the development of new and important fields of discovery in various departments of physical as well as physiological investigation. The scientific world of Paris still canvassed with eagerness the discoveries of *Magendie* relative to venous absorption, and at the very time was receiving new light from the inquiries of the same philosopher into the functions of the nervous system. It was but recently too that the toxicological researches of *Orfila* had been fully appreciated, and he was then at the very height of his fame as a physiologist and as a lecturer. The admirable inquiries of *Edwards* into the influence of physical agents on life were at the time only in the act of being promulgated. The beautiful succession of papers by *Ampere*, describing the progress of his discoveries in Electro-magnetism, was not yet entirely finished, and occupied a large share of the regard of all followers of the purer physical sciences. And in the department of organic chemistry, the career of *Pelletier* and *Caventou*, and of *Robiquet*, which opened up the existence of an entirely new and most interesting class of bodies, the active crystalline principles of the vegetable kingdom, was but recently begun, and had become the general subject of conversation as well as imitation among chemists. A great part of these splendid investigations were brought before the Institute, which Dr Turner regularly frequented, and where he had an opportunity of witnessing the deep respect paid in all quarters to the followers of experimental science, and the certainty with which skill and perseverance on the part of its cultivators were crowned with honours and substantial rewards. To these circumstances, united with the remote and doubtful prospect of advancement in the professional sphere which he first selected, it seems reasonable to ascribe his determination to devote himself to the study of natural science, and more especially of chemistry.

So soon as this resolution was taken, he left Paris in the month of April 1821, for Göttingen; and on arriving in that city had the good fortune to be received as an experimental pupil by the late *Professor Stromeyer*.

The department of chemistry in which this eminent professor had chiefly distinguished himself, was inorganic chemistry and mineral analysis; and he had acquired a high reputation throughout Europe for the fidelity and precision of his researches. His pupil's regards were naturally turned in the same direction.

Dr Turner repaired to the laboratory of Stromeyer at a very early hour every morning; laboured assiduously there the whole day; and for fully two years, except for a short period in the summer of 1822, when he re-visited Bath on account of the death of his surviving parent, he allowed nothing to withdraw him from his chemical occupations. During that term there were few subjects in the inorganic department of chemistry which he had not studied experimentally; while in mineral analysis, the favourite pursuit of his teacher, and one which had been hitherto greatly neglected in Britain, he had attained to such skill and experience, as to find on returning home scarcely any equal in this respect among his countrymen.

Dr Turner's means did not place him above the necessity of professional exertion; and his studies in Gottingen were therefore conducted with a view to his embracing the profession of a lecturer on chemistry. We have the gratification of reflecting, that he, a stranger, and with the whole united kingdom equally open for his choice, fixed upon Edinburgh as the most promising theatre of professional ambition. This resolution was not a little creditable to its inhabitants; for his reasons simply were, that chemistry was nowhere in Britain so generally cultivated, and that nowhere else did there appear to him to be so fair and impartial a field for honourable competition. Such I know were his views; for his plans in regard to this, the most important step in his life, were communicated to me while he was in Gottingen, and I have the pleasure of thinking that my exhortations on the subject determined his choice. In London, the natural and first object of his thoughts, chemistry was at that time far from being so properly appreciated as a branch of medical science or a popular pursuit as in this city. The courses of lectures delivered upon it in the metropolis were ineffectively brief. The subject was not comprised in the regulations of what was then the most important of the corporate bodies who directed medical education. It is not to be wondered at, therefore, that the London students, almost to a man, looked on chemistry as a very subordinate, and many indeed even as a useless, branch of medical study. Only two years earlier, when I was myself a pupil of one of the most populous schools in London, that of

St Bartholomew's, the name of chemistry was scarcely ever mentioned among the students but with contempt; and in a numerous circle of friends I did not know one who had a just idea of its true bearings on medicine as a profession, or who was acquainted with more than the simplest elements of chemical knowledge. Matters are very differently circumstanced now-a-days. Edinburgh can no longer boast of the same superiority. And this change, as will presently appear, was mainly owing to the exertions of the subject of the present memoir.

Departing from Göttingen, where he left many delightful recollections, and an admiring, attached, and steadfast friend in his preceptor, he gathered around him the several members of his family not already settled in life, and arrived in Edinburgh in the autumn of 1823. The subsequent winter was spent in preparing his lectures, the first course of which was delivered in the summer of 1824. From this time he gave an annual six-months' course four years, till he commenced his duties as Professor of Chemistry in the newly instituted London University; and at the same time he regularly received practical pupils, according to a plan introduced a short time before by *Dr Fyfe* into the system of instruction followed in this city.

Like most beginners, Dr Turner at first met with indifferent success as a teacher. The number of his pupils during the first three years was small; and even in his fourth session, when he had established for himself a first-rate reputation as an author, and enjoyed also the fame of his recent appointment to the chemical chair in London, his numbers did not exceed thirty-six. This was partly owing to the firm hold possessed over the students by our distinguished Professor of Chemistry. But in truth Dr Turner had also to contend with intrinsic difficulties. It was not till his larger audiences in London, by removing the temptation to a somewhat conversational mode of lecturing, had insensibly taught him a more easy and sustained delivery than what he first possessed, that he presented the full qualifications of a popular teacher. Eventually, as we all know, he rendered himself a teacher of the first order. Nothing in the shape of public instruction could be more engaging and attractive than his appearances here during the meetings of the British Association in 1834. He then united an easy, mild, and gentlemanly manner, with a smooth, unbroken, animated delivery, perfect fluency and precision of language, uncommon facility in demonstration, and a total absence of all pretension, which reminded many here of the late *Dr Murray* in his happiest days, and which strongly brought to my recollection the lectures of *Gay-Lussac* at the time my friend and I attended him together in 1821.

During his residence in Edinburgh, Dr Turner was not content with studying to become an efficient lecturer. He also laboured to gain the more solid reputation of a chemist; and with what success will appear from the following brief summary of his writings.

His first appearance as a chemical author was in 1824, when he published his " Experiments on the application of Döbereiner's recent Discovery to Eudiometry."* *Professor Döbereiner* had announced a short time before his singular and now familiar discovery of the action of spongy platinum in spontaneously inflaming hydrogen gas in atmospheric air or oxygen. Dr Turner inferred that this observation might be turned to account in Eudiometry; and he proved by a series of apposite experiments, that, by means of a little ball of platinum and clay, oxygen and hydrogen may be made to unite spontaneously, silently, yet swiftly, in the eudiometric tube,—that this agent is a far more delicate mode of discovering and measuring oxygen or hydrogen than the electric spark, being adequate to detect the 100th part of one gas mixed with the other,—and that it gives the exact proportion of these gases in an aëriform mixture. He farther investigated the effect of other gases in preventing the action of the platinum, and settled all the other conditions for its successful eudiometric employment. In fine, he has left but little room for improvement in this department of inquiry; and his eudiometric method has ever since been currently adopted as the most convenient, and most exact in a great majority of instances.

In the same year appeared his " Analysis of Radiated Celestine,"*—an interesting mineral previously known to consist of sulphate of strontia and sulphate of baryta. Dr Turner shewed that the crystallized variety of it consists almost entirely of these two sulphates in such proportion to one another that the mineral forms a definite compound, in which the strontitic salt is to the barytic in the ratio of five atoms to one,—thus adding one more to the instances previously ascertained, and now known to be very numerous, which tend to shew that crystalline minerals, however composite they may be, consist essentially of similar compounds united in atomic proportions as exactly as the constituents of the common earthy, alkaline, and metallic salts.

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* Edin. Phil. Journal, xi. 99, 1824.

His next investigation was one in which I had the pleasure of being his coadjutor. In the autumn of 1824, at the request of the Edinburgh Oil-Gas Company, one of the many joint-stock companies that sprung up in the calamitously speculative years of 1824 and 1825, we undertook a conjunct inquiry into numerous ill-ascertained points respecting the manufacture, illuminating power, and modes of burning, oil and coal gases. And the first result of this inquiry was a paper, published in the spring of 1825, "On the Construction of Oil and Coal Gas Burners, and the circumstances that influence the light emitted by the gases during their combustion; with some observations on their relative illuminating power, and on the different modes of ascertaining it."† The objects of this paper are to show, that the economical combustion of the illuminating gases depends on a single principle being attended to in every part of the construction of gas-burners, namely, that the gases shall be consumed not more vividly than is just sufficient for their complete combustion,—to fix the several points in the construction of burners which are necessary for securing that condition,—to reconcile the conflicting statements of prior experimentalists regarding the relative illuminating power of the gases with a view to settle what that relative power really is,—and, among other incidental objects, to prove that the late *Professor Leslie* was wrong in maintaining that his photometer was exempt from the influence of non-luminous heat, or was applicable to the measurement of differently-coloured lights. Of the merits of this production I cannot presume to speak. But it may perhaps be allowed me to say, that the leading facts and principles contained in it have never been controverted,—and to bear my testimony, that Dr Turner sustained his full share in the whole investigation. Nor can I forbear from adding, that had the company who proposed the inquiry attended to the warnings conveyed in this paper, and more expressly in a subsequent report never made public, they would have been saved a considerable part of the enormous loss to which their recklessness eventually subjected them. With the heedless spirit of the day they erected vast works before determining the scientific data on which their fate was to depend; and with an obstinacy worthy of a better cause, they went on with their operations for two years after they might have seen, from the data at last obtained, that they had embarked in a ruinous undertaking.

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* Edin. *Phil. Journal*, xi. 324, 1824. † Ibid. xiv. 1.

During the same year Dr Turner produced no fewer than five other papers, principally on the subject of mineral analysis. One was an account of some improvements in the preparation of hydriodate of potash,* a salt which was even then coming greatly into demand in consequence of the discovery made five years before by *Coindet* of the virtues of the compounds of iodine in goitre and scrofula. Dr Turner's method, which consists in forming in the usual way a mixture of hydriodate and iodate of potash by means of aqua potassæ, and converting the iodate into hydriodate by a stream of sulphuretted-hydrogen gas, is a very convenient and economical process, which, I believe, is now followed by some manufacturers. Another of his papers published the same year, was his analysis of two mineral species† newly determined by his friend *Mr Haidinger*, then resident in Edinburgh. From external characters Mr Haidinger distinguished the minerals in question as two species of Gypsum-haloide, instead of Selenite and Quartz, for which they had been mistaken. His coadjutor's results were equally satisfactory; for he determined them by analysis to be both hydrated arseniate of lime. About the same period was also published Dr

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* Edin. Med. and Surg. Journal, xxiv. 20. 1825.

† Edin. Journal of Science, iii. 306. 1825.

Turner's "Analysis of Edingtonite," another new mineral species;[*] but here his analysis was incomplete, owing to a deficiency of material.

A few months earlier he had published two successive papers relative to the analysis of mica, and the occurrence of the alkali lithia in this mineral. These were followed up early in 1826, by a third on the same subject; in which he proposes a new method of detecting Lithia in the mineral kingdom. These papers have been considered by mineralogical chemists to be very important in a practical point of view. In 1818, the alkali Lithia had been added to the list of true alkalis by the Swedish chemist *Arfwedson*, and early in 1825 *Gmelin* announced the discovery of it in a particular variety of mica. Soon afterwards, Dr Turner, in his "Analysis of a Mica from Cornwall,"[†] whose composition he determined with his usual accuracy, announced that he had found reason for concluding that lithia is a common constituent of this mineral: and in a subsequent paper, "On Lithion-mica,"[‡] he gives the analysis of three specimens in which he had discovered the new alkali, explains a new process for separating it from potash, and shews that lithia constitutes no less than from two to four per cent. of the micas he had examined. In his third paper on the same subject, namely, "On the means of detecting Lithia in minerals by the Blowpipe,"[§] he shews how its presence may be detected in any mineral by the red colour imparted to the blowpipe flame, when the mineral is fused, sometimes alone, sometimes with a flux of fluate of lime. With the preceding papers may be also arranged a fourth published a few months later, being "an Analysis of two varieties of Lepidolite;"[‖] which he found to consist chiefly of silica, alumina, potash, and fluoric acid, together with the same remarkable ingredient in the lithion-micas, viz. lithia in the proportion of 5½ per cent.

In the same year in which he finished these researches, he brought forward another no less important paper in the same department of chemical science, entitled, "On the detection of Boracic Acid in Minerals by the Blowpipe."[¶] He here shews that, by means of a flux of fluate of lime and bisulphate of potash, boracic acid may be discovered in the minute proportion of 1 per cent. in a mineral, by the green light communicated to the blowpipe flame; and he proves by this method the existence of boracic acid not merely in boracite, datolite, tourmaline, and schorl, in which it had been previously discovered, but likewise in two other mineral species, axinite and colophonite.

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[*] Edin. Journal of Science, iii. 318. 1825.

[†] Ibidem, iii. 137.

[‡] Ibidem, iii. 261.

[§] Ibidem, iv. 113. 1826.

[‖] Ibidem, v. 162. 1826.

[¶] Edinburgh Philosophical Journal, xiv. 124. 1826.

In this year too appeared his announcement of the discovery of iodine, in the mineral spring of Bonnington, in our immediate neighbourhood.* Not long before *Vogel*, *Liebig*, and *Angelini* had excited great interest among chemists, mineralogists, and physicians throughout Europe, by the unexpected discovery of iodine in various continental mineral waters. The announcement of its presence in the water of Bonnington,—a spring, I may observe, of exceeding interest on account equally of its constitution, as of its active properties,—was the first observation of the kind made in Britain; and it has been followed up, as every one knows, by the detection of the same ingredient in many other springs in England by *Dr Daubeny*.

The subsequent year of 1827 was by much the most momentous in Dr Turner's career as an author and as a public man.

In the first place he made public five additional specimens of his experimental skill. The first was a "Chemical Examination of Isopyre,"† a new mineral species recently established by Mr Haidinger, which he found to be composed of silica, alumina, lime, and peroxide of iron. Another was the analysis of a mineral from the hot spring of Oxahver in Iceland, which Sir David Brewster established as a new species under the name of a Oxahverite, and which proved to be almost identical in composition with a previously known species, Apophyllite, as Sir David had inferred from its external characters and optical properties.‡

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* Edin. New Philos. Journal, i. 159, 1826. The experiments on the Bonnington water were made at my request in Dr Turner's laboratory, by one of his pupils, my young friend Mr Copland of Blackwood, who found it to contain iodine.—Edit. of Phil. Journ.

† Edin. New Philos. Journal, iii. 265. 1827.

‡ Edin. Journ. of Science, vi. 118. 1827.

A third was his "Analysis of Sour Clay,"* a very singular mineral production brought from Persia by Lieut. Alexander, where it is used for acidulating sherbet, and which Dr Turner discovered to consist almost entirely of sulphate of lime with free sulphuric acid, derived in all probability from the combustion of sulphur. Of much greater interest to us, however, are his two other papers of the same year, "On the Detection of Antimony in Mixed Fluids,"† and "On the Effects of the Poisonous Gases on Vegetables."‡ The former is one of the most beautiful monographs with which I am acquainted in the whole range of Toxicological Chemistry; and must excite great regret in every admirer of this branch of medical knowledge, that it was not enriched by farther contributions from so accurate and acute an inquirer. In the present paper he shews that in every form in which antimony is at all likely to present itself to the notice of the Toxicologist, it is brought into the fluid condition by means of tartaric and muriatic acids;—that from all states of solution it is thrown down of a peculiar tint by sulphuretted-hydrogen gas;—and that the sulphuret so obtained may be characteristically reduced on the most minute scale by means of a current of hydrogen gas aided by heat. Such is the method now generally followed for discovering antimony in toxicological researches; I have often employed it with great success; and I am wholly at a loss to understand how any man should have found it, as some say they have done, unmanageable. In the investigations upon which his other medico-legal paper is founded, I had again the happiness to be his coadjutor. It is well known that actions at law have repeatedly been brought against manufacturers of black ash, carbonate of soda, and other substances where acid vapours are largely disengaged, on account of the destruction alleged to be occasioned to vegetable life in their vicinity. In one of these actions Dr Turner and I were consulted; and we were requested to supply a gap which had been unaccountably left in all the previous cases of the same nature, by making express trial of the effects on vegetation arising from exposure to the acid vapours disengaged from the manufactories in question. The results proving of interest, as well in a scientific as in a practical point of view, we extended our researches to the gases in general, and arrived at the following conclusions:—viz. That, as in regard to animal life, so also in respect to vegetable life, there are two great classes of gaseous poisons, one comprising the irritant gases and vapours, which injure and destroy vegetables by attacking their external parts or local organs, the other comprising the narcotic gases, which assail vegetable life at its centre and throughout the whole organization of the plant:—that from the former poisons plants may recover with partial local injuries, if they are removed in time into fresh air, while the latter prove inevitably fatal if the slightest sign of their narcotic action be once developed:—And that some of the irritant gases, such as sulphurous and muriatic acid, are so intensely energetic, that in the course of two days the whole vegetation of various species of plants will be destroyed by the most minute quantities, diluted to so great an extent as to be wholly inappreciable by any of the animal senses. These experiments have, I believe, been considered decisive of the general question; and on two subsequent occasions on which I have been consulted as to the question of damage by the same kind of works, I have been able to trace the identical effects on the great scale which my friend and I witnessed in our researches. In one instance the devastation committed was enormous, vegetation being for the most part miserably stunted, or blasted altogether, to a distance of fully a third of a mile from the works in the prevailing direction of the wind.

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* Edin. New Philos. Journ. iv. 243. 1827. † Edin. Med. and Surg. Journ. xxviii. 71. 1827. ‡ Ibidem, 356.

But the year 1827 was marked by two steps of far greater consequence than any yet mentioned: for towards the beginning of it he brought out the first edition of his Elements of Chemistry; and towards the close he was nominated Chemical Professor in the University College, London.

Dr Turner, as I have reason to remember, entered on the laborious task of preparing an Elementary work on Chemistry, with the modesty and diffidence which characterized his whole duct, and which may perfectly characterize the conduct of a man, without in the least degree taking away from its gy or success. He derived great encouragement, however, m the welcome reception met with a short time previously by an elegant little treatise, which he published "On the Laws of Combination and the Atomic Theory," and which was intended to diffuse a knowledge among students of the brilliant light which had been thrown by the atomic doctrines on the chemical constitution of compound bodies. Nowhere is there a clearer sketch to be seen of the subject than in that little work; and it greatly contributed to popularize the study of the atomic composition of bodies, more especially by rectifying an error then prevalent in this country, that the atomic theory was essentially a *hypothesis*, and not, as it really is in a great measure, a legitimate *generalization of facts*.

Of the plan and execution of Dr Turner's Elements of Chemistry, it is unnecessary for me to say one word. Every one is familiar with them: for few have studied chemistry during the last ten years without his Elements in their hands. Permit me, then, simply to express my opinion of the work, which may be done unreservedly and without ostentation, because it coincides with that of every competent judge whom I have heard upon the subject,—namely, that, without the slightest disparagement to several admirable elementary treatises on chemistry in our own language, there is neither in our own nor in any foreign language,—and not in chemistry merely, but even also in any other branch of science, an elementary work which surpasses, or perhaps equals it, for the rare combination of clearness and succinctness of exposition, extent, comprehensiveness, and accuracy of illustration, and ease, purity, and elegance of style. The public have amply expressed the same sentiments; for in the short space of ten years five very large editions have been exhausted. Nevertheless, this work, as I have reason to know,—for the proof-sheets passed through my hands,—was begun and published in the course of little more than twelve months.

Soon after its publication the London University was instituted. The projectors of this institution, looking well to the vastness of their undertaking, and to its probable results, and fully aware that their success was to depend far less on themselves than upon the men whom they should select for teachers, bestowed uncommon pains and exercised unusual caution in choosing their professors. The nomination, then, of Dr Turner to one of the principal chairs in the seminary was highly complimentary to him. The choice, however, was amply justified by the celebrity he had gained as an author and experimentalist abroad as well as at home, and by the lavish, yet well-deserved, encomiums which he carried with him from all his friends in Edinburgh. It need scarcely be added, that the Council's choice was also amply justified by the event: every successive year supplied them with fresh cause for rejoicing in the good fortune which secured for them his services.

Let us here pause for a moment to look back upon the main features of his life down to this last incident in his career. Arrived almost at man's estate with his education neglected or mismanaged, and without giving any indication of what is usually called original talent or genius,—we see him, by the force of a well-regulated and self-directed mind, becoming a diligent and eager student, and speedily acquiring a creditable acquaintance with a profession of exceeding variety and extent in its objects,—then undertaking the study of a science intricate and profound in its doctrines, vast, comprehensive and minute in its details,—one, too, of which he was till then all but utterly ignorant,—and at length in the brief space of six years, overcoming all its difficulties, establishing a high reputation as an original inquirer by numerous experimental researches, earning the name of a popular teacher and esteemed author, and in fine, attaining a lofty station in the scientific world as a chemical philosopher,—a station second, I will venture to say, to that of one individual only in these Islands. How cheering and instructive a lesson is this to the youthful aspirant: how pleasing and impressive a picture for the contemplation of all!

I have thus brought down these biographical sketches to about the middle of Dr Turner's scientific and public life. I have trespassed too much already on your attention to be entitled to detain you with the details of the remainder of it. Besides, our separation at this period put an end to our constant intercourse, and takes away my capability of doing full justice to his future progress. Let me, then, conclude by simply completing the narrative of his writings, and then bringing before you the circumstances of his last illness, which were in every respect full of deep and affecting interest, and in one sense the most instructive of all the incidents in his life.

For some time after his appointment as Professor of Chemistry in London, he continued to exhibit the same untiring zeal in the promotion of his favourite science. In the year 1828 appeared his "Analysis of Tabasheer,"* an "Analysis of two Hot Mineral Springs in India,"† his "Examination of a specimen of Native Iron from the desert of Atamaca in Peru,"‡ and the most elaborate of all his works, his "Chemical Examination of the Oxides of Manganese."§

In his analysis of the Hot Springs of Pinnarkoon and Loorgootha in India, he proves that their solid ingredients are essentially the same with those existing so remarkably in the famous spouting fountains of Geyser and Rykum in Iceland, namely silica held in solution by free soda. His investigations on Tabasheer, the singular siliceous concretion often found in the joints of the bamboo, were undertaken with the view of clearing up the differences of opinion entertained by chemists and physiologists in regard to its real source; and he shows that it consists of pure silica, with a trace of lime and vegetable matter, but without any potash, which had been thought by many to enter into its composition. Consequently the means by which so large a quantity of silica is taken up and subsequently secreted by the plant, remains still a mystery in the economy of vegetable life. The Atamaca iron-ore, the subject of his third analytic paper for the same year, was sent in 1827 by Mr Woodbine Parish, the British Consul at Buenos Ayres, as a specimen of a mineral which was found scattered in large masses and numerous fragments over many leagues of country in the province of Atamaca in Peru. Their structure favoured the idea that their source was meteoric; but their quantity in this view appeared extraordinary. The inference from structure, however, was completely established by analysis; for the specimen proved to consist of metallic iron with six per cent. of nickel and a little cobalt,—a constitution compatible only with a meteoric origin, and almost identical with that of the celebrated meteoric mass found by Pallas in Siberia.

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* Edinburgh Journal of Science, viii. 335. 1828.

† Ibidem, ix. 95. 1828. ‡ Ibidem, 259.

§ Trans. of the Royal Soc. of Edinburgh, xi. 1831. Read Dec. 1827.

Of Dr Turner's chemical researches, that which has been commonly thought the most important by chemists is the last of those I have mentioned as brought forth by him this year, where he has determined the atomic number of manganese and the constitution of its several oxides. He was led to attempt this very difficult investigation, one of the most delicate and complex in the whole range of inorganic chemistry, in consequence of being requested by Mr Haidinger to aid him in an elaborate mineralogical inquiry into the ores of manganese, and finding his purpose obstructed by the doubts prevailing as to the constitution of various compounds of that metal. He therefore undertook a full examination of the subject; in which he first fixes the atomic number, or combining proportion of manganese, from three independent and remarkably concordant results obtained from several analyses of the carbonate, the sulphate, and the chloride; and he then proceeds to ascertain the composition of four admitted oxides, the quantities of the oxygen in three of them being in the ratio of 2, 3, and 4, while the fourth seems a definite compound of the first and third. In a supplement to this inquiry, he shews that several of the oxides exist in nature, both hydrated and anhydrous, constituting some of the mineral species determined by Mr Haidinger; and in a subsequent paper in 1830, his "Chemical Examination of Wad,"* he adds to the number two Cornwall ores known by that name, which he found to be essentially, the one the hydrated, the other the anhydrous, peroxide of manganese.

This whole train of investigation is usually regarded by competent judges as one of the most beautiful and perfect that has yet appeared in the extensive department of inorganic analysis, and, even though its author had done nothing else, would have raised him to the very first rank as an analytic inquirer.

There now remains for notice but one of Dr Turner's original researches,—one which was undertaken in a somewhat remarkable conjuncture; for he appears in it expressly in the character of umpire between two of the greatest of living chemists.

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* Edinburgh Journ. of Science, N. S. ii, 213. 1830.

Chemistry has to thank two distinct classes of philosophers for the extraordinary extension of her modern boundaries. To the one belongs the discoverer, the inventor, the original genius; to the other, the man of minute observation, of cautious judgment, of profound learning, the analyst, the author. To which of these two classes the science stands most deeply indebted, it may be difficult to say. Little progress, however, could be made without both; for the qualities of both seldom pre-eminently co-exist in the same individual. In the latter denomination, Dr Turner was content to rank himself. Throughout his whole scientific life he appears, not as the brilliant discoverer, astonishing the imagination, but as the exact, the cautious observer, satisfying the judgment; and in no capacity has he shone more than in that, which he early and chiefly chose for himself, of an impartial umpire, to fix definitely those boundaries of knowledge which others of more inventiveness had vaguely or dubiously indicated. That he thus duly estimated and happily applied his peculiar gifts, we learn, not more from the internal evidence of his researches, than from his own declaration. "The time is arrived," says he, in the introduction of the paper I am now to describe, "for reviewing our stock of information, and submitting the principal facts and fundamental doctrines of the science to the severest scrutiny. The activity of chemists should now, I conceive, be specially employed, not so much in searching for new compounds or new elements, as in examining those already discovered; in ascertaining, with the greatest possible care, the exact ratio in which the elements of compounds are united; in correcting the erroneous statements to which inaccurate observation has given rise; and in exposing the fallacy of opinions which partial experience or false facts have produced. Considerable as is the labour and difficulty of such researches, they will eventually prove of great importance to chemical science by supplying correct materials for reasoning." *

It was with such views and feelings that Dr Turner commenced his inquiry into the constitution of the Chloride of Barium.

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* Philosophical Transactions, 1829, cxix. 291.

Every one familiar with chemical analysis and its applications to the atomic theory must be aware, that the exact constitution of the chloride of barium is one of the most essential facts in this branch of chemistry, because by its means, and from its composition, are deduced the combining proportions of a very great number of important compound bodies. Its constitution forms, in particular, a material element of the researches detailed by Dr T. Thomson in 1825, in his great work on The First Principles of Chemistry. Unhappily, the correctness of Dr Thomson's experiments on the subject was called in question by Berzelius, the first authority, perhaps, in analytic chemistry; and, still more unhappily, the discussion was conducted by that philosopher with a spirit of acrimony, not less at variance with the great cause of truth than it was unworthy of his own high character, and of the sober dignity of the science he cultivated. It was no feeble proof of Dr Turner's consciousness of his own powers, and of his reliance in the faith reposed by the public equally in his accuracy of research, and in his rectitude of purpose, that he ventured to step forth as arbiter between such men, and in such a cause. He did, however, venture; and he decided the question not merely to the satisfaction of the chemical world, but likewise with such kindliness of disposition and urbanity of manner, as to have lost, in consequence, neither the respect of the one rival, nor the friendship of the other. His paper, considered upon its own intrinsic merits, furnishes a memorable example of exact analytic investigation. He had to determine the true constitution of chloride of silver, and the quantity, both of sulphate of baryta and of chloride of silver, thrown down in a solution of chloride of barium respectively by nitrate of silver and by sulphuric acid. The last of these points he determined by a series of experiments, the results of which differed from one another by no more than 3 in the second decimal figure. In both of the others, the differences in each set of experiments were 2 only in that figure.

This investigation was the first of a projected series on similar subjects; and it was followed up in 1833 in a paper of great experimental research, where he has determined the atomic weights of chlorine, nitrogen, and sulphur, lead, silver and mercury, as well as the composition of several of their compounds.*

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* Philosophical Transactions cxxiv 527. 1833.

Chemistry will long deplore that here his labours were brought to an end in a field so vitally important, and so peculiarly fitted for his cultivation.

Circumstances withdrew him to other occupations. Events, inseparable perhaps from the infancy of so great an institution as that with which he had become connected, entailed a heavy demand upon his time beyond what his labours as a teacher involved. And he considered himself bound to bestow an almost fastidious care upon the successive editions of his *Elements*, to render the work deserving of the high approbation it had received.

How successfully he laboured in both these respects, it is scarcely necessary for me to mention. Let me only observe in respect to the part he performed as a teacher,—that he rendered chemistry, what it had never been before in London, a favourite pursuit among the students of medicine,*—that during the nine years of his incumbency he stood in the first station among the Lecturers in the University College and in the metropolis,—and that, if I am not misinformed, his college, on some trying occasions,—especially one where its very existence was in danger,—owed the deepest obligations to his honourable character, straightforward dealing, firmness of purpose, and conciliating disposition.

Ere long a fresh and more grievous cause gave farther interruption, and gradually put an end, to his scientific labours. Originally of a sound and even hardy constitution, he nevertheless taxed it beyond its powers.† The first indication to this effect occurred in the spring of 1834, in the shape of slight stomach complaints and other collateral ailments. Though these were greatly alleviated by a visit he paid to Edinburgh during the meeting of the British Association, they recurred on his return to London; and in the ensuing winter they increased so much that he resolved to spend the entire summer of 1835 in a continental excursion. His health, though thus greatly improved, was far from being altogether reinstated; and the confinement and labours of the succeeding college session undermined it still more. Another summer of complete relaxation in the country brought a deceitful tranquillity, but no thorough restoration; and his friends looked with alarm to the necessity of his again resuming his professional toils. His complaints were vaguely marked, consisting chiefly of irritability of the stomach, with a sense of fulness and oppression in the head; which were successively conceived by himself and various medical friends to arise from irritation in the stomach, inflammation in the duodenum, diseased liver, an affection of the brain, and obstruction in the lacteal system. He appears, however, to have been scarcely at any time apprehensive about the issue; for while he was visibly to his friends declining slowly but steadily, he constantly flattered himself with fresh prospects of amendment. So late as towards the close of January, in the last letter I received from him, he spoke cheerfully of his returning health, and of his intention to take two hours a-day from the relaxation he had enjoyed throughout the early part of the winter. A few days afterwards, I received the intelligence that he had been seized with the epidemic influenza, then prevailing in London; and notwithstanding the most assiduous and anxious care on the part of his medical colleagues, his enfeebled frame sunk under this new invader. He expired on the 12th of February, in the forty-first year of his age.

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* The numbers of his pupils in University College between the sessions 1828–29 and 1836–37 inclusive, were 144, 145, 169, 199, 198, 238, 225.

† Many of Dr Turner's later friends have imagined he was originally of infirm and delicate constitution. But this is altogether a mistake. Though very spare in form, he was, before he settled in London, remarkably healthy and vigorous,—a hardy pedestrian, a strong swimmer, and very seldom ailing.

The cause of his long illness was discovered after death to have been chronic inflammation of the mucous coat of the stomach and duodenum, proceeding to ulceration; and the immediate cause of death was serous effusion, and extensive hepatization in both lungs.

His body was interred in the new cemetery at Kelsall Green, about five miles from London; and the funeral was attended by a large assemblage of friends, and followed by upwards of 300 tudents of the college, who requested permission to pay this bute of respect to his memory. Seldom, indeed, has a teacher ceived such sincere and unaffected proofs of attachment from is pupils. Many of them continued to wear mourning afterwards as if for a relative; and they have resolved to present a bust of their favourite Professor to the institution where they profited by his instructions.

Few words are needed to depict a character, like Dr Turner's, in which open sincerity of mind and simplicity of heart were the predominating qualities. He was the very soul of honour in every act and thought: without this quality indeed his reputation as a faithful and exact experimentalist never could have been established. Himself undeviating in probity, and keenly alive to defects in the character and conduct of others, he was nevertheless gentle and indulgent to all. Of warm feelings, yet in constant possession of temper,—energetic in action and thought, yet mild and winning in his deportment,—unpretending, yet without reserve, in his address and manner,—he seldom failed to gain at once the esteem and confidence of those with whom he came in contact. During the many years we passed in one another's society, I have reason to believe that he never made an enemy, and never lost a friend. In his domestic relations he was a pattern of all that is good. The junior members of his father's family resided constantly with him, and received from him all a father's care and tenderness, as well as a brother's warmest love. Scarcely a scheme did he plan without their interests forming an essential element of it: In every recreation they were his never-failing companions. Throughout all the relations of life, with his pupils, his colleagues, his friends, and the world at large, he exhibited the same kindness of feeling, the same disinterested conduct. But in no respect was his character more strongly marked than by his Christian principles and practice. At all times his mind was deeply imbued with the feeling of true religion; and, far from allowing the pursuit of science to withdraw him from religious contemplation, which has unhappily been the case with too many of its cultivators, his faith grew in purity with his knowledge. In his latter years his favourite recreation was the study of the history and principles of the Christian church; and his great delight was to follow this study in company with the members of his family. Under such mental discipline, and the hallowing influence of long and increasing illness, his mind was purified of what remained of this earth's corruption. That his faith was as pure as the faith of man can be, we have the testimony of his intimate friend and former colleague, the Vicar of Saint Bride's, and the internal evidence of his own deportment on his deathbed. That deathbed was in many respects so solemn and instructive, as to have been taken by his reverend friend for the subject of a most eloquent and impressive funeral address to his congregation,*—from which I need make no apology for borrowing the leading particulars of his closing scene.

During his final illness he had shewn throughout the utmost resignation and cheerfulness. When at length told for the first time of his danger, he desired to receive the sacrament with his brother and sisters, in presence of the members of his household. Having communicated, he called his brother to his bedside, and bidding him feel his pulse, "'Is it not," said he, "perfectly calm?" "It is," was the reply. "Then what can make it so at such an hour? What but the power of religion? Who but the Spirit of God?"

After some time spent in occasional conversation of the same purport, symptoms at last came on whose indications he knew full well. Painfully struggling for utterance, he recovered his speech for a little, and spoke kindly and cheerfully to his relatives of his condition. "I could not have believed," he said, "that I could be happy on my deathbed: I am content my career should close." The last effort of reason was to answer soon afterwards the question put by an anxious relative,—"Is not Christ as good as his word?" "Yes," he faltered, "Quite;" and with these words he became insensible and soon expired.

Analysis of the Scales of the Fossil Gavial of Caen in Normandy. By A. Connell, Esq. F.R.S.E. &c.

Having lately procured from Mr G. B. Sowerby of London, a specimen of these scales, which are particularly described by Cuvier in the Ossemens Fossiles, I submitted it to a chemical examination, with the view of comparing the result with that afforded by fossil scales of fishes.

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* The Philosopher, entering like a child, into the Kingdom of Heaven. A Sermon, preached on the occasion of the death of Edward Turner, M.D., &c. By the Rev. Thomas Dale, Vicar of St Bride's, London. Taylor and Walton. 1837.

The calcareous slab from which the specimen in question was taken, contained several of these gavial scales, the largest of which were about two inches square. Their appearance corresponded closely with the account of them given by Cuvier in the following extract, particularly as respected their great thickness, and the cavities on their surface. "They differ from those of living crocodiles more than any other part of the skeleton, and this crocodile of Caen was undoubtedly the best mailed of the whole genus. The scales are very thick, rectangular, thinner towards the edge, and their whole external surface is hollowed into little hemispherical cavities, of the size of a lentil or pea, and pressed against one another."* The scales examined were not carinated, and had probably belonged to the sides of the animal. Their structure was partially laminated; but they had also acquired a texture somewhat crystalline, possessing a fine-grained fracture, a certain degree of translucency on the edges, and a hardness equalling that of fluor, and much exceeding that of any fossil fish scales which I had ever seen.

The methods of analysis were the ordinary ones applicable to such a case, and were the same as those formerly published. The constituents of the Caen scales were found to be

Phosphate of Lime, with a little fluoride of calcium,	78.59
Carbonate of Lime, ...	12.53
Sulphate of Lime, ...	1.96
Phosphate of Magnesia, ...	.11
Chlorides of Potassium and Sodium, . .	.74
Oxide of Manganese, ...	.45
Siliceous Matter, ...	.37
Water, ...	5.07
	99.82

One or two considerations are suggested by this result.

In the *first* place, It is manifest, that these scales were originally of the nature of bone, and, in all probability, entirely analogous to the osseous scales of fishes; and hence the presence or absence of bone-earth in such fossil relics can be of no service in determining whether they had belonged to saurian animals or to fishes, as I at one time, founding on the usual views of chemists respecting the nature of recent saurian scales, had thought might have been the case.

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* Ossemens Fossiles. tom. v. part ii. p. 139. Pl. VII. fig. 41.

In the *second* place, it may be a matter of inquiry, which is left to any one who may be inclined to follow out the subject, what differences existed between the dermal covering of extinct saurian animals, and that of existing species of this description; for if there is any foundation for the view that the flat scales of recent crocodilean animals consist of horny or other highly azotized matter, and contain little or no bone-earth, then it is clear that they differ from those at least of *some* extinct animals of this nature.

The very limited number of undoubted fossil saurian scales which I have had an opportunity of seeing, or of which I have read particular descriptions, agree in the above-mentioned external character of having their surface hollowed into hemispherical cavities of considerable size, in proportion to the magnitude of the scale. This observation applies to the Caen scales; to those of the crocodile of Argenton, which I have never seen, but which are described by Cuvier in the Ossemens Fossiles; and to those of the Monheim Gavial of Soemmering, in the British Museum. This character I have also observed in the carinated dorsal and bony-looking scales of some large recent crocodiles; but whether it would afford a means of distinguishing fossil saurian from fossil fish scales, is a point which I do not pretend to decide, and leave to others who have more ample information, and opportunities of observation on the subject.

On the Chemical Composition of Clay-Slate. By HERMANN FRICK.*

THE chemical composition of clay-slate has hitherto been but little examined, and the investigations that have been made have yielded very different results. The following table contains all the analyses with which I am acquainted, viz.:

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* Tom. v. part ii. p. 168.

From Poggendorf's Annalen, 1835.

1. Of a thin lamellar clay-slate, by D'Aubuisson.
2. Of the clay-slate of Dunmeniss in Downshire, by Stokes.
3. Of the clay-slate of Gaggenau in Baden, by Holtzmann.
4. Of the clay-slate of Nieiderselters in Nassau, by Wimpf.

	(1.)	(2.)	(3.)	(4.)
Silica,	48.6	59.4	64.34	79.17
Alumina,	23.5	17.4	23.90	10.42
Oxide of iron,	11.3	11.6	9.70	6.27
Oxide of Manganese,	0.5	—	—	—
Lime,	—	2.1	—	—
Magnesia,	1.6	2.2	—	—
Potash,	4.7	—	—	—
Carbon,	0.3	—	—	—
Sulphur,	0.1	—	—	—
Water,	7.6	6.4	2.22	2.78
	98.2	99.1	100.16	98.64

All these analyses, as it appears, are of varieties of clay-slate occurring in the transition series of rocks; and the small agreement among them, renders it probable that clay-slate is not a simple mineral like mica, as has often been considered the case, owing to its supposed passage into mica-slate, but rather that it constitutes a very finely mixed, only apparently homogeneous mountain-rock. I have, therefore, made some analyses to ascertain if clay-slate can be separated by treatment with acids into a decomposable and an undecomposable component ingredient, just as C. Gmelin has shewn to be the fact with phonolite and basalt, and Berzelius with meteoric stones; and as I have fully succeeded in this attempt, I have performed complete analyses of several varieties of clay-slate.

Each clay-slate was examined in a double manner; first by separating its two component portions by means of muriatic acid, and then submitting each portion separately to analysis; and, secondly, by treating and analyzing the substance as a whole, when of course the result ought to correspond with that of the combined processes in the first analysis.

I shall begin by describing the method employed in the last kind of analysis, as it required the separation of the whole constituent parts of the clay-slate; and I shall afterwards point out in what respect the first differed from it.

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* This slate is employed for paving the streets of Paris.—Edit.

Analysis of Clay-slate considered as a whole.

The clay-slate was decomposed by fusion with carbonate of potash, the melted mass was digested with diluted muriatic acid, and the solution evaporated to perfect dryness. The silica was separated in the usual way from the dry mass. A stream of sulphuretted hydrogen was passed through the liquid, which had been separated by filtration from the silica; and a very inconsiderable precipitate of sulphuret of copper was thus formed, which, as the quantity was too small, was only dried, very strongly heated, and determined as oxide of copper; while in the filtered solution the oxide of iron was converted into the peroxide by nitric acid. The alumina and oxide of iron were precipitated by ammonia, then dissolved in muriatic acid, and separated by means of caustic potash. The alkaline solution was acidulated, and the alumina precipitated by ammonia. The oxide of iron not having been dissolved by the caustic alkali, was dissolved in muriatic acid, and precipitated by succinate of ammonia. The succinate of iron was heated, and the iron determined as peroxide. The liquid separated by filtration from the succinate of iron, and which contained a minute quantity of magnesia, was added to the liquid containing magnesia that I obtained after the separation of the lime. The lime was separated by oxalate of ammonia, from the solution which I obtained after the precipitation of the alumina and oxide of iron by ammonia; and the oxalate of lime was exposed to a red heat, and thus converted into carbonate of lime. The magnesia was precipitated by phosphate of soda. The alkali could not be determined in these analyses. The quantity of water was ascertained by the loss sustained during heating. As, however, it appeared during the solution of the clay-slate in acid, that it contained a minute portion of carbonate of lime, the carbonic acid was also included in this loss of weight; but the quantity was determined in the second analysis, and the amount of water was corrected. The quantity of carbon, a substance which is contained in all the clay-slates I have examined, could not be ascertained, but it was, as we shall afterwards see, included in the calculation of loss. After a long continued strong red heat the dark colour of the clay-slate remained unchanged.

Analysis of Clay-slate, by separation into its component portions (Gemengtheile).

The clay-slate, reduced to fine powder, was several times digested with moderately concentrated muriatic acid, and the solution filtered; and, in order to separate the silica of the portion decomposable in acids from the undecomposable component part, the still moist filter with the residue was then boiled with a concentrated solution of carbonate of soda in a platinum vessel, and the liquid obtained was filtered while hot. The remainder, which had the same colour as clay-slate, became white when exposed to a high temperature, a sufficient proof that the colour of the clay-slate proceeded from a mixture of an organic substance, and that this colouring matter is contained only in the portion undecomposable by acids. After the powder was exposed to a red heat it was weighed, and from its weight was calculated the weight of the part undecomposable by muriatic acid. The alkaline solution, separated by filtration from the insoluble powder, was saturated to excess with muriatic acid, then evaporated to perfect dryness, and the silica, as above, separated from the dry mass by dissolving the latter in water. The solution obtained by digesting the powdered clay-slate with muriatic acid, was tested for copper by means of sulphuretted hydrogen, but no precipitate appeared. Alumina and oxide of iron were thrown down by ammonia, and separated, as in the previous analysis. The portion of magnesia still contained in the solution, separated by filtration from the succinate of iron, was precipitated by phosphate of soda. The lime was, as formerly, precipitated from the solution filtered from the oxide of iron and alumina. In order to determine the magnesia and the alkali in the solution separated from the lime, the liquid was evaporated to dryness, and for a long time cautiously heated in a weighed platinum crucible until all the sal-ammoniac was volatilized. There remained behind chloride of magnesium and chloride of calcium, which were converted into sulphuric salts by sulphuric acid. The salts were then weighed and dissolved, the sulphuric acid was precipitated by acetate of baryta, the solution evaporated to dryness, and the dry mass heated to redness in a platinum vessel. The heated mass, which consisted of carbonates, was treated with hot water; and thus carbonate of potash was dissolved, and was then filtered from the remaining undissolved carbonates of baryta and magnesia. The dissolved carbonate of potash was evaporated to dryness, and the dry mass converted into sulphate of potash and weighed. The carbonates of baryta and magnesia left undissolved by the water were dissolved in muriatic acid, decomposed by sulphuric acid, and the resulting sulphates of magnesia and baryta filtered, evaporated to dryness, exposed to a red heat, and weighed. The united weight of the sulphate of potash and the sulphate of magnesia agreed with that which I had obtained before the separation of the two. I could not discover the presence of soda in the sulphate of potash.

The component portion of the clay-slate which was not decomposable in muriatic acid, was strongly heated with carbonate of baryta after being separated from the silica of the soluble portion. The carbonic acid was separated in the usual manner, the baryta precipitated by sulphuric acid, and the analysis performed in the same way as in the previous one.

The different varieties of clay-slate which I have analyzed are the following:—

1. From Goslar in the Harz.
2. From Benndorf near Coblenz.
3. From Lehsten in Thuringia.

They are all from the transition series, coloured greyish-black by carbon. They split into thin slates, and belong to the kind termed roofing-slate. The appearance of clay-slate before the blowpipe is similar in all varieties. If held with platinum pincers, and exposed to a very strong heat, clay-slate is melted at the edges into a dark green glass. In a retort it yields water. With soda a black glass is the product. It is acted on with difficulty by phosphate of soda; but, by the separation of the silica, a colourless glass is formed, which, when cooled, acquires a yellow tint. With borax the result is the same, though the colour of the cooled glass is more intense.

A larger mass of clay-slate from Benndorf, melted in a platinum crucible, forming a dark green glass resembling obsidian, full of small cavities, and with a brown covering on the surface.

1. *Analysis of Clay-Slate as a whole.*

	From Goslar.	From Benndorf.	From Lehsten.
Silica, ...	60.03	62.83	64.57
Alumina, ...	14.91	17.11	17.30
Oxide of Iron, ...	8.94	8.23	7.46
Magnesia, ...	4.22	1.90	2.60
Lime, ...	2.08	0.83	1.16
Oxide of Copper, ...	0.28	0.27	0.30
Water and Carbon, . . .	5.67	4.06	4.62
Potash and Loss, ...	3.87	4.17	1.99
	100.00	100.00	100.00

2. *Analysis of the Component Portions of Clayslate.*

A. By treatment with acids there was decomposed :—

28.98 p. cent. 26.46. 23.61.

which consisted of

Silica, ...	23.01	22.39	22.16
Alumina, ...	16.19	19.35	21.48
Oxide of Iron, ...	20.19	27.61	27.57
Magnesia, ...	11.60	7.0	8.29
Lime, ...	4.63	2.42	1.26
Potash, ...	1.96	2.37	1.65
Water, Carbonic Acid, & loss,	22.32	18.86	17,59
	100.00	100.00	100.00

B. After treatment with acids there remained undecomposed :—

71.02 p. cent. 73.54 76.59

which consisted of

		Quantity of Oxygen.		Quantity of Oxygen.		Quantity of Oxygen.
Silica, ...	74.98	38.95	77.06	40.03	77.68	40.35
Alumina, . . .	14.32	6.68	15.99	7.46	15.74	7.35
Oxide of Iron, . .	4.94	1.37	1.53	0.46	1.22	0.37
Magnesia, . . .	1.48	0.57	0.57	0.12	1.32	0.51
Lime, ...	0.78	0.20	0.33	0.09	0.60	0.46
Oxide of Copper, .	0.36	0.07	0.19	0.03	0.40	0.08
Potash, ...	3.38	0.57	3.94	0.66	3.14	3
Carbon and Loss, .	0.26	...	0.39	...	...	
	100.00	...	100.00	...	100.10	

If we assume that all the lime found in 2 A is mixed with the clay-slate as a carbonate of lime, and so determine the quantity of carbonic acid by the amount of the lime; then, by deducting this from the loss sustained by heating in No. 1, and by afterwards determining the water, we obtain the following as the result of the first analysis.

		Quantity of Oxygen.		Quantity of Oxygen.		Quantity of Oxygen.
Silica,	60.03	31.18	62.83	32.64	64.57	33.54
Alumina,	14.91	7.05	17.11	8.45	17.30	8.07
Oxide of Iron,	8.94	2.74	8.23	2.52	7.46	2.28
Magnesia,	4.22	1.63	1.90	0.73	2.60	1.00
Lime,	0.51	0.14	0.24	0.07	0.46	0.12
Oxide of Copper,	0.28	0.04	0.27	0.05	0.30	0.06
Water,	4.45	3.95	4.03	3.58	4.08	3.62
Potash, loss, & carbon,	3.87	...	4.17	...	1.99	...
Carbonate of Lime,	2.79	...	1.22	...	1.24	...
		...		...		...
	100.00	...	100.00	...	100.00	...

The analysis of the portion separable by acids :—

Silica,	23.01	11.95	22.39	11.63	22.16	11.51
Alumina	16.29	8.44	19.35	9.03	21.48	10.03
Oxide of Iron,	20.19	6.46	27.61	8.46	27.57	8.45
Magnesia,	11.60	4.49	7.00	2.70	8.29	3.20
Potash,	1.96	0.33	2.37	0.40	1.65	0.27
Water,	15.98	14.20	15.75	14.29	17.31	15.38
Carbonate of Lime,	8.22	...	4.29	...	2.25	...
		...		...		...
	97.25	...	98.76	...	100.71	...

If we calculate the composition of the whole according to the results of the analysis of the component portions, we obtain the following as the amount of the chemical constituents.

Silica,	59.92	62.59	64.58
Alumina,	14.89	16.88	17.10
Oxide of Iron,	9.03	8.42	7.43
Magnesia,	4.42	2.26	2.29
Lime,	0.51	0.24	0.16
Potash,	2.75	3.31	2.93
Water,	4.45	4.03	4.08
Oxide of Copper,	0.25	0.13	0.30
Carbonate of Lime,	2.43	1.22	0.53
Carbon and Loss,	1.35	0.92	0.00
	100.00	100.00	100.00

I made some experiments to ascertain if the relation of the component part decomposable by oxides, to that of the part undecomposable, be the same in every specimen of clay-slate, and I obtained the following results.

Goslar.	Lehsten.
30.53 : 69.47	25.31 : 74.69
29.73 : 70.27	24.48 : 75.52
28.98 : 71.02	23.61 : 76.39

In all these analyses the iron was regarded as the peroxide. I ascertained by direct experiment in the case of the portion decomposable by acids, that the iron is contained in it only in the state of peroxide. To determine this the clay-slate was dissolved by muriatic acid in a small flask, which was closed by a tightly fitting glass-stopper. The solution diluted with water yielded with caustic potash the usual brown precipitate of oxide of iron. This point could not be determined by exact experiment in the portion indecomposable by acids; but it is probable that the iron is there also in the state of peroxide.

It results from these analyses, that the transition clay-slate, from the great formation constituting the Rhenish slate series, the transition-rocks of the Hartz, and those of Thuringia, and indeed most probably all transition clay-slate, can be separated by treatment with acids, into two, and if the small admixture of carbonate of lime be taken into account, into three component ingredients. The composition of the first-mentioned two portions is not similar, but the chemical constituents are the same, though extremely different in their relative proportions. We have also found that the proportion of the part decomposable in acids, to that of the portion indecomposable, is not the same in true varieties of clay-slate which were examined; and, indeed, not even in the different fragments of one and the same variety, though the differences are not very great. But the distinctions are sufficiently great to enable us to ascertain, with some probability, if the quantities of oxygen of the separate chemical constituents of the clay-slate stood in a simple relation. On comparing, however, the numbers placed next the results of the analysis, with the amount of oxygen in the ascertained chemical constituents, we do not find this to be the case. It is when we consider the composition of the clay-slate as a whole, that we have the nearest approach to a simple proportion; here it almost appears as if the quantity of oxygen of the silica were three times as great as that of the bases, and that the clay-slate contained neutral combinations of silica; although the proportion of the silica is throughout too large, and the differences too great, to be ascribed to errors in the analysis. It is therefore apparent, that the clay-slate of the transition series is not a simple mineral, an opinion proved beyond a doubt by the result of treatment with acids; but the fact of the composition of the mineral ingredients into which clay-slate can be separated, not corresponding with the doctrine of fixed proportions, proves that the clay-slate is not to be regarded as a compound of two simple minerals, but as the product of the decomposition of other mountain rocks; but the close agreement in composition, of the varieties of clay-slate belonging to one and the same formation, shews that, during the formation of these varieties, extremely similar circumstances must have existed. We must not, however, extend our conclusions to the rock termed primitive clay-slate. Its very intimate connection with mica-slate prevents us from forming any other opinion than that it is either a mass of pure mica, or a mixture of quartz and mica. In order to draw a satisfactory conclusion on this subject, it would be necessary to institute a separate investigation, for which purpose a perfect analysis of mica-slate itself would be an indispensable preliminary.

Annual Report on the State of the Useful Arts, ordered by the Society of Arts for Scotland. By EDWARD SANG, Esq. F. R. S. E. Vice-Pres. Soc. Arts.*

On the Progress of Exactitude in the Manufacture of Machines.

IT is a trite remark, but one that cannot be too often repeated, that art and science go hand in hand.

There are arts, the mere practice of which implies extensive scientific acquirements, not perhaps in the actual operator, but in the person who originated them; and there are branches of science which never could have been prosecuted, until the improved state of the arts had placed instruments in the hands of the philosopher.

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* Read 7th December 1836, 11th January 1837, and 21st June 1837.

Thus all the modern brilliant discoveries in electro-magnetism would have been unknown, if the manufacture of wire had still been carried on by means of the forge hammer; while the science of optics, and all the sciences which indirectly have benefited by it, would have been uncultivated but for the arts of glass-making and glass-grinding; nay, without these, that collosal collection of facts, chemistry itself, would have been—to be.

Nor are the physical sciences alone dependent on the state of the arts; Metaphysics, the science of sciences, has been affected thereby; thus the celebrated doctrine of the *absolute plenum*, the absurd elements of which are religiously preserved in the Elements of Euclid, arose from improvements in the construction of geometrical instruments; improvements sufficient to bring into view the variations which take place in the dimensions of solids, but insufficient to trace them to their source.

On the exactitude attainable in workmanship depends mainly the improvement of the sciences; and, therefore, every contrivance for the more precise construction of instruments, becomes a matter of intense interest to the student of physics. What prevents, at this moment, the navigator from determining readily his position at sea?—the want of an instrument powerful and steady enough to bring into view the moons of Jupiter; the want of a chronometer which may beat without error. We are yet ignorant of the distance of the fixed stars, and why? because we have no instrument capable of reading off angles to the thousandth part of a second.

In every department of human knowledge we experience like obstructions from the want of sufficient precision. In chemistry, the contested questions as to the component parts of substances, arise from the inaccuracy of weighing; from the difficulty of confining the developed gases, or of examining the entire amount of the products. Exactitude is indeed our war-cry against the concealed intricacies of natural phenomena.

We should then have expected all the recent improvements in exact workmanship to have emanated from the laboratory of the philosopher. But such has not been the case. The improvements in the art of turning, for instance, have almost all of them had reference, not to the turning of the axis of an astronomical circle, or to the perfect elaboration of its limb; but to the vulgar and common-place matters of constructing a sugar-roller, or of fitting steam-tight the cover of a cylinder; and while the slide-rest and self-acting lathe are tools indispensable in the machine-shop, they are but beginning to assert their right to enter the shop of the philosophical instrument-maker. The exactitude of fitting exhibited in a common spinning-frame, or in a self-acting mule, equals, if it do not surpass, that which is attained even in our superior theodolites and circles; and the most beautiful of our air-pump fittings vanish before the exquisite adaptation of our steam-engines.

This inversion of the relative positions of tool-making and instrument-making, must attract the attention of every one who is conversant with the tools used, and methods followed by the two classes of workmen; and becomes particularly conspicuous when we enter the larger and more recent of the tool manufactories. There we find planing-engines, and lathes of enormous dimensions, the finish and workmanship of which would honour our best observatories, pursuing silently, and almost unattended, their laborious avocations; cutting down material till its surface rival in smoothness and finish the parts of the machines themselves; we find them stopping, and reversing, and renewing their actions, with such composure and sagacity, that one is almost tempted to investigate the position of their sensorium.

In this paper I propose to lay before the Society a short and general account of what seem to me the latest and most remarkable improvements in these two instruments, the Planing-engine and Turning-lathe.

The parts of all machines are made to move upon each other; now, it is well known to the geometer, that there are only three kinds of lines which possess the property of gliding along their counterparts; these are, the straight line, the circumference of e circle, and the helical spiral.

The motions, then, of all machines, are performed by help of slides, axes, or screws; and thus the consideration of the arts of formation of machines resolves itself into three principal heads; the first treating of the planing-engine, the second of the turning-lathe, and the third of the screwing-engine.

The planing-engine is now so generally known, and so universally employed, that I might be excused by most of those who take an interest in the proceedings of such a Society as this, from giving a description of its general action. It may be well, however, to glance at the nature of the process.

The planing-engine is used for making straight and flat work; for this purpose a large and heavy plate is fitted to slide accurately upon a frame. Over the path of that plate there is fixed a second slide carrying the cutting tool. The material to be operated on is secured on the moving plate, so that when the plate is drawn along, the cutter removes part of the substance, making a straight cut; the sliding plate is then returned, provision being made to raise the tool during this motion, and a second cut is made parallel to, and close by the first; the tool having, previously to the renewal of the motion, been displaced a little by its regulating screw. Such is the general action of the machine; it has its inconveniencies.

The most serious inconvenience, that which is not merely noticed, but felt, by a person watching its performance, is, that the whole time of the back-motion is lost; and, along with the time, a considerable amount of power. This inconvenience has been removed by an ingenious contrivance of Mr Whitworth of Manchester. He places the tool in a cylindric holder, and thus provides for its being turned round. As soon as a cut has been made in one direction, and while the action is being reversed, this cylinder, and along with it the tool, is turned half round, so that a second cut is made while the plate is returning. In this way, by the reversion of the tool, at each reversion of the plate, the cutting goes on without interruption.

For work not requiring the utmost degree of precision, this back-cutting must undoubtedly be of immense advantage; but where very great delicacy is needed, the method seems somewhat objectionable; the strains on the different parts of the substance operated on are reversed, compression being converted into distension, distension into compression; so that a source of inaccuracy proportional to the flexibility of the substance is introduced. Such extreme cases, however, seldom occur, as the rigidity of each portion of a machine ought to be sufficient to prevent any perceptible error of this kind.

It is very easy to imagine other methods of cutting during both motions of the plate, but Mr Whitworth's has this advantage, that as the axis of the cylinder is adjusted perpendicular to the cross slide, the tool must sink to the same depth each cut: were this not the case, the surface would be reeded, the cuts being alternately deep and shallow.

The second inconvenience of the planing-engine is most apparent when we attempt to urge it by hand. On account of the great weight of the plate, immense friction is caused on its slides, the pressure needed to overcome which sometimes far exceeds that exerted merely in cutting. To remove this, friction-rollers have been placed beneath the plate; and although these be liable to several disadvantages, such as the difficulty of accurate lineation, and the risk of dust, planing-engines fitted in this way, and carefully managed, seem to perform well. One which I examined in Mr Whitworth's, produced work not at all inferior in precision to that of sliding plates. Engines of this kind, however, want that principle of self-correction which often renders a used superior to a new instrument.

These are the two principal considerations connected with the economy of the planing-engine. There are several other matters, rather, however, belonging to a detailed description, than fitted for a general report. These I shall pass over, excepting in so far as to notice a very beautiful adaptation of the screw to the driving of the engine. The plate is usually drawn backwards and forwards by means of a strong chain wound on a barrel: the plate of the engine which I have just mentioned was driven by a screw. In order to obtain sufficient rapidity, the separation between the threads of the screw must be large; were such a screw to work in a common box, the friction would, on account of the obliquity, be very much augmented; to provide against this, Mr Whitworth has placed four friction-rollers, two bearing on the one side, two on the other side of the thread (I think a four-inch thread), so that, by this combination of rollers, the friction is so much reduced, that a very slight pressure suffices to put the instrument in motion; the application of the screw gives peculiar facilities for the reversion of the motions.

The complete turning-lathe contains all the elements of the screwing-engine, so that it is hardly worth while to examine them apart; indeed I am not aware of any recent improvement in the one which does not apply to the other.

In using the ordinary turning-lathe, the cutter is held in the hand, and rested against a strong obstacle. By the motion of the hand the cutting edge is brought in contact with the revolving substance, and part of that substance is cut away; but whether the cut be circular or not depends altogether on the steadiness of the hand. If the lathe-spindle turn very slowly, as in turning large pieces of cut-iron, even the most practised and patient workman would find it impossible to keep the cutter so steady in its place as to present considerable errors in the work; but if the revolution be very rapid, as in turning soft woods, there is scarcely time given for any important change in the position of the tool. Hence the slide-rest was most readily adopted where large masses were operated on. This instrument retains the tool securely in its place during the revolution, and thus produces work much more precise than could possibly be obtained from the use of the hand-tool. The slides and screws of the slide-rest enable the operator to move the cutter, so as to give to the work any required form. If the work is to be cylindric, conical, or flat, he has only to adjust one of the slides to the proper angle in order to obtain the wished for result. But even when the surface wanted is generated by the revolution of a curve, the experienced operator, with a well-adjusted slide-rest, will produce work scarcely surpassed in sweetness of outline by that of the hand-tool.

The transition from the slide-rest to the self-acting lathe is simple and natural; we have only to connect the screws of the slide-rest with the spindle of the lathe, so that the motion of the one may occasion motion in the others. By this means the application of the hand is entirely avoided, and even the effects of unequal rapidity in the motion of the tool destroyed. To complete the change, the bed of the lathe is converted into the lower slide of the slide-rest.

The most beautiful specimen of the self-acting lathe which I have met with, is in the works of Mr Whitworth, works which are indeed crowded with well-made and excellently designed instruments.

This lathe, somewhere about twenty feet in length, is fully finished in all its parts, and kept in a style which shews that the workman is conscious of the merits of his tool. Along the bed of it there runs a screw, accurately divided, the work of some two or three months; this screw is seldom stopped. It works in a split box, which can be opened and shut at will, thus allowing of the instantaneous disengagement of the plate which carries the cutting tools. This plate carries a small wheel which the screw works, so that the axis of this wheel is constantly turning round. By applying the hand to the winch attached to this axis, the screw, whether revolving or stationary, is converted into a rack, and the slide is brought at once opposite any part of the work. The split-box is then engaged, and the cutter moving slowly along the bed, cuts either the thread of a screw, or the surface of a cylinder, according to its form and relative rapidity of motion.

If, however, it be wished to turn a flat surface, the cutter must be moved at right angles to the bed of the lathe. For this purpose, the split-box being disengaged, the small wheel already mentioned is connected with the axis of the transverse screw of the slide-rest, and by a simple arrangement may be made to move the tool either outwards or inwards. Thus the workman, by merely arranging the engaging and disengaging apparatus, giving his commands as it were to the machine, produces what kind of work may be required.

In this particular lathe, the leading screws and other gearing are carefully concealed beneath the plates, so as to remain uninjured by the fragments which fall from the work; at the same time, those parts by which the gear is changed, are brought out ready for the hand, not properly of the workman, but of the director of the work.

By means such as these, it must be evident that we are able to produce work incomparably more exact than by the hand; yet this circumstance would hardly have led to the general introduction of such contrivances into machine-shops. The mercantile consideration that the work is performed at a fraction of the expense, was the talisman that caused the change. Slide-rests and self-acting lathes have been derided, (what good thing has not been derided?) as the lazy man's tool; as destructive of the manual dexterity of our workmen.

In order to vindicate them from these aspersions, I shall proceed to consider first the comparative facility of production obtained by them; secondly, the sources of minute and perplexing error which still remain; and, lastly, the influence which the use of such instruments must have on the physical and mental condition of the men.

The casual visitor of the work-shop forms a very incorrect notion of the relative rapidities of performance of the slide-rest and hand-tool. The expert hand-turner has those tools which he is likely to need laid beside him in order; he lays down one and takes up another almost with as much ease as he removes a tool from one part of the work to another, and hence the mind of the on-looker is filled with the ideas of industry, dexterity, and rapidity; nor is it erroneously so filled.

But when he proceeds to the slide-rest and watches the succession of operations, he frets at the time spent in unscrewing the tool-holder, in selecting, adjusting, and fixing the tool; he sees the process of cutting begun, and perceiving at once the uniformity and certainty of the operation, he withdraws his attention until the commencement of a new cut offer matter of additional interest. He is then tempted to exclaim against the waste of time in altering the tools, and to hint that the hand-turner would have had the job finished while the director of the slide-rest was selecting his cutters. The quantity of monotonous work has been overlooked.

Such is the source of the objection thrown against the slide-rest for the slowness of its proceeding. Experience soon removes this objection.

The first estimate of the relative amounts of skill required is as far wide of the truth. A stranger to the art of turning has only to take a tool in his hand and apply it against the work, to be feelingly convinced that more skill than he had even imagined is necessary. The roughness of the cut, if indeed he succeed in making a cut at all, indicates that not the turning-lathe but the workman has the skill; but how different with the slide-rest! The tool once placed, we have only to turn the winch to make work as good as that produced by the experienced artisan. It is the slide-rest, not the man, that operates; and this is clear, for we may even employ the steam-engine to drive the leading screw of the slide-rest.

This conclusion, however, is hastily reached. The steam-engine cannot determine on the acuteness of the edge, the proper inclinations of its planes to the axis of the lathe, nor of the projection which the cutter may have beyond its support. These considerations require brains, and an active exercise of that material organ of thought.

The hand-turner feels from the nature of the cut whether he have placed his tool in the most advantageous position, and by a motion which appears natural, adjusts the direction of the instrument. Great skill and considerable practice are needed for the accomplishment of this motion; but we must not judge that skill and practice are not needed by the *slider.*[*] His tool once placed, remains there; while the finish of the work depends essentially on the proper placing of the cutter; but the inconveniencies of an improper position are not experienced by himself; he must divine their existence from the appearance of the material, or from the sounds emitted by the machine; and he can only remove these by a change in the form or position of the cutter. In the sharpening and placing of the cutter, then, is his whole skill shewn; these accomplished, he leaves the advancement of the tool to be performed by any agent capable of exhibiting the required amount of physical power—yet the rapidity of this advancement he must also regulate.

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[*] Technically, we say to slide a cylinder, not to turn it.

That the slide-rest is capable of producingan increased amount of certain kinds of work, will be allowed by all. Thus, suppose that we have to turn a vast number of plugs for stop-cocks. The receptacles for these plugs have been all brotched out to one size and one taper, by the use of one and the same brotching-tool. To turn a plug by hand is difficult and tedious. The preservation of a perfect straight line in the taper is impossible; and the regulation of the degree of taper a matter of repeated trial. But with the slide-rest, the accuracy of lineation and the true angle of obliquity are at once obtained; and the rest once set, nothing has to be attended to, save the giving of the proper diameter to one or other end of the cone. In such a case as this the amount of work produced by the same number of hands has been augmented four, five, six, and even ten times; nor is this all; the operations have been performed with prodigiously increased precision; so much so, that perhaps a thousand times the time had been spent by the hand workman in the vain attempt to rival it.

Multitudes of examples of this kind occur in practice; as in the turning of the spindles and other parts of spinning machines, where thousands of copies of the same form are needed; and where these copies must not merely look tolerably well, but where they must be so exact that one may be placed instead of the other. In such cases, I have said, nobody disputes the advantage of the slide. It is where a single article has to be made that it is supposed to be disadvantageous. Here the whole trouble of adjusting falls on one piece of work, instead of being subdivided among hundreds.

But what is really the case in hand-turning? The drawing of the intended article is laid down, and measurement after measurement is made from it; the callipers are applied repeatedly, the aim of the turner being not to turn too much off, but to bring, by repeated essays, the work to the required size and form. The eye cannot transfer with certainty the dimensions from the flat paper to the solid material, and hence a waste of time, far more tedious than that spent in adjusting the cutter of the slide-rest. And after all, unless it be an affair where precision is not aimed at, where delicacy of outline and of curvature are all important, the finished hand-turning is any thing but complete; and smoothing papers have to be applied to perfect the surfaces.

When three or four copies are needed, the hand-turner is more at ease; yet even his first copy occupies more time than the original piece; for now he has to give a much more complete resemblance, as the eye detects much more easily a disagreement between two solids, than between a solid and its orthographic projection. The third, and fourth, and fifth performances, however, are gone through with great rapidity, and if many copies be required, each may not average the third or fourth part of the time spent on the first.

Contrast this process with the operation of the slide-rest. A cut is made, and the dimensions of the material left ascertained by the callipers; a simple subtraction tells how much more must be taken off, and the divisions on the head of the leading screw give at once the proper advance to the tool, so that the work is made of the required size safely and directly. There need here be no guessing as to how far we must go, no fear that the material is destroyed by an over cut; and in this way a drawing is copied with a rapidity and safety that can hardly be equalled by the best hand-turners.

The only thing in which the slide-rest appears to fall decidedly short of the hand-tool is in the last delicate finish of a curved outline; and there its inferiority is palpably apparent. Yet in the formation of the approximate outline, it is not inferior to the hand-tool; and in the cases of brass or iron is perhaps superior to it. For the removal of the ridges left between the successive cuts, the hand must be employed, and not the hand merely, but after it, grinding stones, and smoothing and polishing papers.

The introduction of the slide-rest has brought into view sources of inaccuracies in the turning-lathe, that before were hardly dreamt of.

The edge of the tool wears as the work goes on, and hence a deviation from the true form. Thus, in the example before cited of turning stopcock plugs, the tool, after many cuts, has been worn down and blunted, so that the last plugs, supposing the screw of the slide-rest always brought to the same mark, will be larger than the first one. To palliate this evil a very simple plan is followed. The plugs, or other articles, are all made roughly to about one size, very nearly what they are to be when finished; by this means the rough exterior, which generally occasions the greatest wear of the tool is removed. The prepared plugs are then submitted to the finishing process, the cutter for which having no great extent of metal to go through, is worn but very slowly. This is a very obvious source of inaccuracy; but there is another, less apparent but more constantly annoying. It is the flexibility of all the materials, that on which we are operating, as well as that of which the parts of the lathe are composed.

Considerable pressure is needed to cause the cutting edge to enter the substance operated on, and to force it through that substance. This pressure must be resisted on the one hand by the slide rest and shear of the lathe, on the other by the material cut. The part of the slide-rest, lathe-bed, tool-holder, and cutter, being subjected to pressure, are bent, and the extent of this bending is proportional to the pressure employed; so that the tool, instead of describing the right line, which it ought to describe, moves over a line somewhat removed from that, and not even straight, excepting in the rare case of perfectly homogeneous turning material. The extent of this flexure can only be reduced by giving greater strength to the parts of the turning apparatus; and as this flexure is more seriously felt in small objects, it follows, that to make even minute work accurately, we must have a lathe of great weight and strength.

This deviation of the cutting edge from its proper place, may always be resolved into two deviations, one horizontal, that is, from or towards the axis, and the other vertical. The first of these deviations goes directly to affect the radius of the work, and in that which is therefore most to be guarded against. This deviation is not always *from* the axis, but is frequently towards it; indeed, if the tool be properly formed, it ought with most substances to be inwards, as the peculiar form which gives clean and easy cutting also induces a tendency to drag the tool still deeper. The horizontal deviation of the cutting edge is produced by the twisting of the shear, by the bending of the upright part of the rest, and by inaccuracies in the fitting of the slides; the methods of palliating these evils (for they can never be entirely removed) are apparent and simple. But, if the lathe be constructed, and if all these evils exist, the skill of the workman is then called into active exercise, in devising a tool which may give occasion to as little inward or outward pressure as possible; this problem is not one of very easy solution.

The pressure is resisted on the other hand by the material under operation; and here we have a source of error very difficult of removal.

The pressure on the material, exactly in the same way, resolves itself into two, one horizontal tending to bend the substance, and thus to affect its diameter; the other vertical, tending to twist it. The torsion of the spindle and material is unavoidable, it is indeed the measure of the force (excluding friction) necessary to drive the lathe. The flexure, however, is extraneous, and may be palliated or removed by attention to the forms and actions of the parts; the formation of the cutting edge being the most important.

The influence of the flexure is most felt when the material has an elongated form, as with the piston-rod of a steam-engine, or the cylinder for a screw. In such cases a prop attached to the slide-rest, is brought to support the work almost opposite the cutter, but on the turned portion of the rod; in this way the flexure is almost completely prevented, and a heavy cut can be taken, however long the rod may be; but such a contrivance is convenient only in the case of cylinders; and is barely practicable in the case of a slightly tapered cone. In other cases stationary props must be employed to subdivide the inconvenient length.

That part of the pressure which goes to twist the spindle, and which ought to be by far the greater part of it, introduces that peculiar effect which is called criddling or waving, a phenomenon not at all capricious as many would imagine, but governed by very exact laws. The avoiding of criddle is a very important point with all turners; but it is much more easy to avoid it in appearance than in reality; for often when the surface presents no traces of it to the eye, the ear will have detected the sound which it occasions, while the microscope reveals it in all its realities, though minute in the breadth of the waves.

Now simple, mechanical, as the management of the slide-rest may appear, it requires no small progress in a knowledge of the laws of mechanics, and no little ability in the application of these laws, to guard against these minute, yet complicated and important sources of error; and therefore, so far from considering the slide-rest an instrument likely to lull asleep the mental powers of the workman, I regard it as a powerful incentive to the exercise of mind. And of this indeed I am assured, that he who has mastered the difficulties of slide-rest turning, and has learned to shape his tools for the work they have to perform, will thence pass easily and rapidly to the art of manipulating hand-tools. Accustomed to study the principles of cutting, it will be to him a simple matter to put these principles in practice.

All our knowledge of physical phenomena is derived from experience. Mere experience indeed will collect but a small quantity of information; the intellect must seek out the results of experience, must compare, arrange, and classify them, before any thing worth calling knowledge is attained to; yet still experience lays the foundation of science, and affords the materials on which the mind operates. That such is the case with all our knowledge of material phenomena is, I think, evident on an inspection of any one of or all the branches of physics. The algebraist knows of the existence and properties of prime numbers from experience alone; he cannot shew why they are or ought to be. As little can the geometer tell why the three angles of a trigon make together half a revolution; or the mechanician deduce *a priori* the law of equilibrium, or of accumulated motion. So much, indeed, are our ideas tied to the course of our experience, that even those poets who have ventured into the unknown land of spirits; have been utterly unable to shake off the impressions of materiality—

> Gli occhi in giuvolse,

says Tasso, as if the Author of all must have the eyes of a man.

If, then, knowledge be so essentially experimental; and if, for the development of knowledge, the study of the relations of phenomena be needed, it must be that that branch of manufacture which most calls for the exercise of mind, which substitutes mental energy for brute force, is well fitted for expanding the intellect. The due exercise of the physical organism is indeed essential to a healthy state of mind; yet those workmen whose employment consists more in directing the energies of steam than in muscular exertion, and whose business requires the combination of skill and patience, stand in the most favourable situation as intellectual beings.

The mere direct application of physical strength in overcoming resistance for which that strength is adequate, leads slowly, if at all, to the expansion of the mind. But when the resistance to be overcome far exceeds the strength of the individual, or is of a kind to which his organs do not directly apply, the powers of the intellect are rapidly called forth.

Thus, we find among quarrymen, a thorough knowledge of the properties of the lever, and considerable acquaintance with the doctrine of the centre of gravity; and among those workmen who are employed about the railways in adjusting the positions of the waggons, an intimate acquaintance with the laws of momentum. In witnessing the readiness with which their knowledge is brought to bear on the matter in hand, we are very apt to say, "It is merely the result of practice, of repeated trial." True! and what other was the knowledge possessed by Galileo, Newton, or Lagrange? The difference is not in *kind*, but in *degree*.

Suppose that a person whose mind has been trained to investigation, who has learned the expeditious methods of inquiry now in common use, were to begin his apprenticeship in any of these employments, no one can doubt for a moment, that while his previous tuition will lend him prodigious assistance in accumulating knowledge; the practical or rather actual illustration continually before him, will give his mind a familiarity with the mechanical laws which he could never have acquired from logical deductions.

On these general grounds, then, I think it reasonable to infer, that the manipulation of planing engines and slide-lathes, is highly conducive to the intellectual improvement of the workmen. This opinion is fully borne out by a specific examination of the processes.

Let us take up the problem "to construct a turning lathe," and follow its general solutions, by hand and by machine.

The shear or bed of the lathe has first to be formed. By hand,—we must line it as well as we can, and then carefully file to these lines. At first no difficulties present themselves, and the coarse files are used freely; but when the finishing files are in use the difficulties crowd upon the workman, and the more expert he is, the more accurate his ideas of what is needed, he sees the more clearly the utter impossibility of ever making, in this way, satisfactory work. He in fact has recourse to every means of avoiding dependence on the hand and eye, and hence the very objectionable form of shear, with a broad flat face, has come to be preferred, because that face can be ground upon a lap, and then affords a more satisfactory guide in the filing of the edges. To attain eminence in this kind of work, the artist must devote all his attention to the balance and motion of his hand, and must learn to suit it to the curvature of his file. The direct fruits of this attention are indeed in themselves good; but the collateral results are not always so. On the one hand, seeing the utter impossibility of removing every defect, the workman is apt to say it is well enough, and thus to content himself with imperfect execution. That mental satisfaction which arises from the thorough finish of a piece of work is lost, and the standard of excellence is lowered. On the other hand, if, by long and assiduous attention, the workman excel in the difficult art of flat-filing, he exults in the attainment of mere physical prowess, and the sequence is too often extravagance and penury: it is for mental acquisition peculiarly to gratify without over elating the vanity. If ever the idea of filing a body round has been entertained, it has long since been given up in favour of turning; it is time, then, that flat-filing should give place to the planing engine.

Contrast this tedious, inexact and disheartening process with the operation of the engine. The best form of the guides can be assumed without a consideration of the difficulty of reaching the parts; and, if the engine be carefully arranged, there is no fear of the accuracy of the performance. The workman, instead of hewing away the iron by mere physical strength, forms his tools so as to produce the required effects. The self-feeding apparatus relieves him from the drudgery even of attendance, excepting when intellect is essential to the process. The mental as well as the physical energy is thus economised, and opportunity is left for their full employment at those times when they are really needed. The proper fastening of the work upon the plate, so that the pressure exerted in cutting may not bend the parts, the action of particular cutters, and the effects of short or long strokes upon the engine itself, fall under peculiar notice, and the possibility of attaining to greater and greater precision is exhibited.

If we go into a similar comparison of the other parts of the process, results exactly analogous are reached, and it appears that the self-acting or engine tools raise the standard of excellence, and tend in a corresponding manner to elevate and improve the mind.

That the kind of employment in which we may be engaged exerts a powerful influence on the morals and on the intellect, is tolerably evident on a review of the state of the labouring classes. We find that particular degrees of intellectual and moral development are attached, with variations indeed in individual cases, to different employments; and that the greatest diversities occur among those tradesmen who have the lightest physical and the heaviest mental labour.

The whole progress of the human mind resolves itself into the lessening of bodily exertion. It is this which constitutes the distinction between the present state of society in Britain, and that state in which it existed before the Roman conquest. The mere contrivance of the facility of production is not indeed the whole cause of civilisation, but it is that one which leaves to every other room for operating; and as such is its influence on the human race generally, it is to be expected that the like influence will be produced on any particular class. Contrast or a moment the state of the machine-makers before the inven- on of the slide-rest and planing engine with their present state; ney left their work in the evening wearied, or rather worn out, by their exertions at the lathe; and, as the payment was generally per piece, the most industrious, those most likely in other circumstances to rise in life, were the most jaded; the least able, perhaps the least disposed, to any further exertion. For the mass of them the charm of stimulating drinks was thus great, and those men who, from their position, ought to have set the best example to the other classes set perhaps the worst. But now the men leave their work tired perhaps, but not worn out; they are able to apply themselves to their rational recreation and improvement. The stimulus of intoxication is no longer necessary for keeping them awake; and is, accordingly, falling into disuse among them.

The situation of the master of a work is confessedly more favourable to intellectual improvement than that of his workman. Now, the use of steam-impelled tools places the men in a position analogous to that of the owner, it makes them directors of power rather than exerters of it; and thus, on every hand, we are led to the conclusion that such tools tend directly to the improvement of society.

In large factories where many copies of the same thing are made, or where the nature of the work allows a fair estimate, the men are paid by the quantity of work; a deduction being made as the rent of the tool. In this way the workman becomes, to a certain extent, a capitalist. He embarks, as it were, in a mercantile speculation, with this in his favour,—that he has no risk, and that industry and skill is sure to be rewarded. The machine which he rents can at the utmost move over a certain distance; the planing engine proceeds at a certain rate, and so does the driver of the side-lathe. It would seem, then, that no opportunity is left for the exercise of skill; that do what the workman will, a fixed amount of work will be turned out. This, however, is by no means the case, for considerable time may be saved by a dexterous shifting of the tools or of the work, and, what is still more important, by the proper shaping and keeping of the tools much deeper cuts may be made. In fact, the difference of skill may enable one workman to produce from 50 to 100 per cent. more work than another could turn out with the same instrument. The skilful artist is thus encouraged to proceed by the conviction that while his employer reaps the advantage of his skill in improved workmanship, he himself reaps a still greater personal profit. The factory which once required sharp overseers to keep the men at their work, which exhibited a continual conflict between the interests of the employer and of the employed, now shews a harmony between those interests; the only opposition of interest being in the bargain of how much per piece. In this manner the depressing influence of continual jealousy is avoided and greater harmony and more perfect co-operation secured between the different classes of society.

In my previous reports I have not hesitated to advert to the truth that the cultivation of the morals, taste, and intellect of all classes, is essential to the improvement of society; and have insisted particularly in favour of the more numerous classes, and perhaps I do not go out of my way in the present paper, by drawing the attention of the Society of Arts to the facilities which this city affords to the improvement of their tastes.

With praise-worthy zeal in a good cause, some of our public bodies have thrown open their institutions. Still, a commencement merely has been made in this course, and scarcely a public place is accessible to the workman; in many cases the charge, and in almost all the hour, presenting serious obstacles. It is indeed matter of deep regret that our museum which contains objects of such intense and varied interest should remain shut against us; and that while Scotland furnishes her share of the expenses of the British Museum, the trifling compensation which would make ours patent to the public is denied us; and that thus the meritorious labours of our eminent Professor of Natural History are, comparatively, locked up.

Impressed with the importance of this subject, I would earnestly recommend to the Society, that in founding a museum of models, arrangements should be made for affording, at hours convenient to the workmen, the freest admission, and every facility for taking sketches and dimensions; that this Society should, as far as they are able, follow in the footsteps of, or even precede the public-spirited societies to which I have alluded.

Thermometric Tables; Barometric Tables; Tables of Toises, French Feet and Inches, Metres and Millimetres; Tables of English Feet and Inches. Calculated by GEORGE ATKIN, Esq.

THERMOMETRIC TABLES, Nos. I. II. III.

THE following Tables are for the purpose of converting the degrees of the three thermometric scales into one another. When the quantity to be converted is a whole number, its value is ascertained by simple inspection of that table in which the proposed scale stands in the first column. Thus, to convert + 85 R. into degrees of Fahrenheit and Centigrade, by Table I. we find:

+ 85 R. = 223.25 F. and 106.25 C.

Again, if —65 C. were the given number, we find in Table II.

—65 C. = —85.0 F. and —52.0 R.

Lastly, to convert + 42 F. into the corresponding scales, in Table III. we find:

+ 42 F. = + 4.44 R. and + 5.55 C.

If there be fractional parts annexed to the proposed number, by having recourse to the table of proportional parts, the value of the part is ascertained, and must be added to the number found in the body of the table; observing that, when the signs are alike, the sum is taken when unlike the difference. The signs of the fractional parts are the same in all the scales.

Example.

1. Convert + 44.2 R. into degrees of Fahr. and Cent.:

By Table I. + 44 R. = + 131.00 F. + 55.00 C.
+ 0.2 = 0.45 0.25
44.2 R. = 131.45 F. 55.25 C.

2. Convert —40.4 R. into degrees of Fahr. and Cent.:

—40 R. = —58.00 F. —50.00 C.
— 0.4 = — 0.9 — 0.5
—40.4 R. = —58.9 F. —50.5 C.

3. Convert —9.6 R. into degrees of Fahr. and Cent.:

—9 R.	=	+ 11.75 F.	—11.25 C.
—0.6	=	— 1.35	— 0.75
—9.6 R.	=	+ 10.40 F.	—12.00 C.

If the proposed quantity extends to hundredths or thousandths of a degree, it may be converted by shifting the decimal point of the proportional part either one or two places to the left. For example:

4. Convert + 52.246 C. into degrees of Fahr. and Reaum.:

+ 52 C.	=	125.6 F.	41.6 R.
+ 0.2	=	0.36	0.16
+ 0.04	=	0.072	0.032
+ 0.006	=	0.0108	0.0048
+ 52.246 C.	=	126.0428 F.	41.7968 R.

BAROMETRIC TABLES, Nos. IV. V. VI.

1. Required the value in French inches and lines, and English inches, of a column of mercury 729.7 mill. in height. By Table IV.

		Inch.	Lin.	
729 mill.	=	26	11.16 French.	28.701 English inch.
+ 0.7	=		0.315	0.0275
729.7	=	26	11.475	28.7285

Tables V. and VI. are used in the same manner, and serve for the conversion of French inches and lines and English inches into one another, and also into millimetres. By these tables a conversion may be made at a half or third of any of the numbers, by doubling or trebling the given quantity, and then a half or third of the result will be the quantity required.

TABLES VII. to XIV.

Are for converting the different French and English measures into one another, and are used in the same manner as the preceding.

TABLE I.

Fahr.	Cent.	Reau.	Fahr.	Cent.	Reau.	Fahr.	Cent.	Reau.	Fahr.	Cent.
—103.00	—75.00	+1	+34.25	+1.25	+62	+171.50	+ 77.50	+123	+308.75	+153.75
100.75	73.75	2	36.50	2.50	63	173.75	78.75	124	311.00	155.00
98.50	72.50	3	38.75	3.75	64	176.00	80.00	125	313.25	156.25
96.25	71.25	4	41.00	5.00	65	178.25	81.25	126	315.50	157.50
94.00	70.00	5	43.25	6.25	66	180.50	82.50	127	317.75	158.75
91.75	68.75	6	45.50	7.50	67	182.75	83.75	128	320.00	160.00
89.50	67.50	7	47.75	8.75	68	185.00	85.00	129	322.25	161.25
87.25	66.25	8	50.00	10.00	69	187.25	86.25	130	324.50	162.50
85.00	65.00	9	52.25	11.25	70	189.50	87.50	131	326.75	163.75
82.75	63.75	10	54.50	12.50	71	191.75	88.75	132	329.00	165.00
80.50	62.50	11	56.75	13.75	72	194.00	90.00	133	331.25	166.25
78.25	61.25	12	59.00	15.00	73	196.25	91.25	134	333.50	167.50
76.00	60.00	13	61.25	16.25	74	198.50	92.50	135	335.75	168.75
73.75	58.75	14	63.50	17.50	75	200.75	93.75	136	338.00	170.00
71.50	57.50	15	65.75	18.75	76	203.00	95.00	137	340.25	171.25
69.25	56.25	16	68.00	20.00	77	205.25	96.25	138	342.50	172.50
67.00	55.00	17	70.25	21.25	78	207.50	97.50	139	344.75	173.75
64.75	53.75	18	72.50	22.50	79	209.75	98.75	140	347.00	175.00
62.50	52.50	19	74.75	23.75	80	212.00	100.00	141	349.25	176.25
60.25	51.25	20	77.00	25.00	81	214.25	101.25	142	351.50	177.50
58.00	50.00	21	79.25	26.25	82	216.50	102.50	143	353.75	178.75
55.75	48.75	22	81.50	27.50	83	218.75	103.75	144	356.00	180.00
53.50	47.50	23	83.75	28.75	84	221.00	105.00	145	358.25	181.25
51.25	46.25	24	86.00	30.00	85	223.25	106.25	146	360.50	182.50
49.00	45.00	25	88.25	31.25	86	225.50	107.50	147	362.75	183.75
46.75	43.75	26	90.50	32.50	87	227.75	108.75	148	365.00	185.00
44.50	42.50	27	92.75	33.75	88	230.00	110.00	149	367.25	186.25
42.25	41.25	28	95.00	35.00	89	232.25	111.25	150	369.50	187.50
40.00	40.00	29	97.25	36.25	90	234.50	112.50	151	371.75	188.75
37.75	38.75	30	99.50	37.50	91	236.75	113.75	152	374.00	190.00
35.50	37.50	31	101.75	38.75	92	239.00	115.00	153	376.25	191.25
33.25	36.25	32	104.00	40.00	93	241.25	116.25	154	378.50	192.50
31.00	35.00	33	106.25	41.25	94	243.50	117.50	155	380.75	193.75
28.75	33.75	34	108.50	42.50	95	245.75	118.75	156	383.00	195.00
26.50	32.50	35	110.75	43.75	96	248.00	120.00	157	385.25	196.25
24.25	31.25	36	113.00	45.00	97	250.25	121.25	158	387.50	197.50
22.00	30.00	37	115.25	46.25	98	252.50	122.50	159	389.75	198.75
19.75	28.75	38	117.50	47.50	99	254.75	123.75	160	392.00	200.00
17.50	27.50	39	119.75	48.75	100	257.00	125.00	161	394.25	201.25
15.25	26.25	40	122.00	50.00	101	259.25	126.25	162	396.50	202.50
13.00	25.00	41	124.25	51.25	102	261.50	127.50	163	398.75	203.75
10.75	23.75	42	126.50	52.50	103	263.75	128.75	+164	+401.00	+205.00
8.50	22.50	43	128.75	53.75	104	266.00	130.00			
6.25	21.25	44	131.00	55.00	105	268.25	131.25			
4.00	20.00	45	133.25	56.25	106	270.50	132.50			
—1.75	18.75	46	135.50	57.50	107	272.75	133.75			
+ 0.50	17.50	47	137.75	58.75	108	275.00	135.00			
2.75	16.25	48	140.00	60.00	109	277.25	136.25			
5.00	15.00	49	142.25	61.25	110	279.50	137.50			
7.25	13.75	50	144.50	62.50	111	281.75	138.75			
9.50	12.50	51	146.75	63.75	112	284.00	140.00			
11.75	11.25	52	149.00	65.00	113	286.25	141.25			
14.00	10.00	53	151.25	66.25	114	288.50	142.50			
16.25	8.75	54	153.50	67.50	115	290.75	143.75			
18.50	7.50	55	155.75	68.75	116	293.00	145.00			
20.75	6.25	56	158.00	70.00	117	295.25	146.25			
23.00	5.00	57	160.25	71.25	118	297.50	147.50			
25.25	3.75	58	162.50	72.50	119	299.75	148.75			
27.50	2.50	59	164.75	73.75	120	302.00	150.00			
29.75	—1.25	60	167.00	75.00	121	304.25	151.25			
+32.00	0.00	+61	+169.25	+76.25	+122	+306.50	+152.50			

PROPORTIONAL PARTS.

Reau.	Fahr.	Cent.
0.1	0.225	0.125
0.2	0.450	0.250
0.3	0.675	0.375
0.4	0.900	0.500
0.5	1.125	0.625
0.6	1.350	0.750
0.7	1.575	0.875
0.8	1.800	1.000
0.9	2.025	1.125

TABLE II.

Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.
—75	—103.00	—60.0	—2	+28.4	— 1.6	+71	+159.8	+56.8	+144	+291.2
74	101.2	59.2	1	30.2	0.8	72	161.6	57.6	145	293.0
73	99.4	58.4	0	32.0	0.0	73	163.4	58.4	146	294.8
72	97.6	57.6	+ 1	33.8	+ 0.8	74	165.2	59.2	147	296.6
71	95.8	56.8	2	35.6	1.6	75	167.0	60.0	148	298.4
70	94.0	56.0	3	37.4	2.4	76	168.8	60.8	149	300.2
69	92.2	55.2	4	39.2	3.2	77	170.6	61.6	150	302.0
68	90.4	54.4	5	41.0	4.0	78	172.4	62.4	151	303.8
67	88.6	53.6	6	42.8	4.8	79	174.2	63.2	152	305.6
66	86.8	52.8	7	44.6	5.6	80	176.0	64.0	153	307.4
65	85.0	52.0	8	46.4	6.4	81	177.8	64.8	154	309.2
64	83.2	51.2	9	48.2	7.2	82	179.6	65.6	155	311.0
63	81.4	50.4	10	50.0	8.0	83	181.4	66.4	156	312.8
62	79.6	49.6	11	51.8	8.8	84	183.2	67.2	157	314.6
61	77.8	48.8	12	53.6	9.6	85	185.0	68.0	158	316.4
60	76.0	48.0	13	55.4	10.4	86	186.8	68.8	159	318.2
59	74.2	47.2	14	57.2	11.2	87	188.6	69.6	160	320.0
58	72.4	46.4	15	59.0	12.0	88	190.4	70.4	161	321.8
57	70.6	45.6	16	60.8	12.8	89	192.2	71.2	162	323.6
56	68.8	44.8	17	62.6	13.6	90	194.0	72.0	163	325.4
55	67.0	44.0	18	64.4	14.4	91	195.8	72.8	164	327.2
54	65.2	43.2	19	66.2	15.2	92	197.6	73.6	165	329.0
53	63.4	42.4	20	68.0	16.0	93	199.4	74.4	166	330.8
52	61.6	41.6	21	69.8	16.8	94	201.2	75.2	167	332.6
51	59.8	40.8	22	71.6	17.6	95	203.0	76.0	168	334.4
50	58.0	40.0	23	73.4	18.4	96	204.8	76.8	169	336.2
49	56.2	39.2	24	75.2	19.2	97	206.6	77.6	170	338.0
48	54.4	38.4	25	77.0	20.0	98	208.4	78.4	171	339.8
47	52.6	37.6	26	78.8	20.8	99	210.2	79.2	172	341.6
46	50.8	36.8	27	80.6	21.6	100	212.0	80.0	173	343.4
45	49.0	36.0	28	82.4	22.4	101	213.8	80.8	174	345.2
44	47.2	35.2	29	84.2	23.2	102	215.6	81.6	175	347.0
43	45.4	34.4	30	86.0	24.0	103	217.4	82.4	176	348.8
42	43.6	33.6	31	87.8	24.8	104	219.2	83.2	177	350.6
41	41.8	32.8	32	89.6	25.6	105	221.0	84.0	178	352.4
40	40.0	32.0	33	91.4	26.4	106	222.8	84.8	179	354.2
39	38.2	31.2	34	93.2	27.2	107	224.6	85.6	180	356.0
38	36.4	30.4	35	95.0	28.0	108	226.4	86.4	181	357.8
37	34.6	29.6	36	96.8	28.8	109	228.2	87.2	182	359.6
36	32.8	28.8	37	98.6	29.6	110	230.0	88.0	183	361.4
35	31.0	28.0	38	100.4	30.4	111	231.8	88.8	184	363.2
34	29.2	27.2	39	102.2	31.2	112	233.6	89.6	185	365.0
33	27.4	26.4	40	104.0	32.0	113	235.4	90.4	186	366.8
32	25.6	25.6	41	105.8	32.8	114	237.2	91.2	187	368.6
31	23.8	24.8	42	107.6	33.6	115	239.0	92.0	188	370.4
30	22.0	24.0	43	109.4	34.4	116	240.8	92.8	189	372.2
29	20.2	23.2	44	111.2	35.2	117	242.6	93.6	190	374.0
28	18.4	22.4	45	113.0	36.0	118	244.4	94.4	191	375.8
27	16.6	21.6	46	114.8	36.8	119	246.2	95.2	192	377.6
26	14.8	20.8	47	116.6	37.6	120	248.0	96.0	193	379.4
25	13.0	20.0	48	118.4	38.4	121	249.8	96.8	194	381.2
24	11.2	19.2	49	120.2	39.2	122	251.6	97.6	195	383.0
23	9.4	18.4	50	122.0	40.0	123	253.4	98.4	196	384.8
22	7.6	17.6	51	123.8	40.8	124	255.2	99.2	197	386.6
21	5.8	16.8	52	125.6	41.6	125	257.0	100.0	198	388.4
20	4.0	16.0	53	127.4	42.4	126	258.8	100.8	199	390.2
19	2.2	15.2	54	129.2	43.2	127	260.6	101.6	200	392.0
18	0.4	14.4	55	131.0	44.0	128	262.4	102.4	201	393.8
17	+ 1.4	13.6	56	132.8	44.8	129	264.2	103.2	202	395.6
16	3.2	12.8	57	134.6	45.6	130	266.0	104.0	203	397.4
15	5.0	12.0	58	136.4	46.4	131	267.8	104.8	204	399.2
14	6.8	11.2	59	138.2	47.2	132	269.6	105.6	+205	+401.0
13	8.6	10.4	60	140.0	48.0	133	271.4	106.4		
12	10.4	9.6	61	141.8	48.8	134	273.2	107.2		
11	12.2	8.8	62	143.6	49.6	135	275.0	108.0		
10	14.0	8.0	63	145.4	50.4	136	276.8	108.8		
9	15.8	7.2	64	147.2	51.2	137	278.6	109.6		
8	17.6	6.4	65	149.0	52.0	138	280.4	110.4		
7	19.4	5.6	66	150.8	52.8	139	282.2	111.2		
6	21.2	4.8	67	152.6	53.6	140	284.0	112.0		
5	23.0	4.0	68	154.4	54.4	141	285.8	112.8		
4	24.8	3.2	69	156.2	55.2	142	287.6	113.6		
-3	+26.6	—2.4	+70	+158.0	+56.0	+143	+289.4	+114.4		

PROPORTIONAL

Cent.	Fahr.
0.1	0.18
0.2	0.36
0.3	0.54
0.4	0.72
0.5	0.90
0.6	1.08
0.7	1.26
0.8	1.44
0.9	1.62

TABLE III.

ahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.
-100	—58.66	—73.33	—35	—29.77	—37.22	+30	—0.88	—1.11	+95	+28.00	+35.00
99	58.22	72.77	34	29.33	36.66	31	0.44	0.55	96	28.44	35.55
98	57.77	72.22	33	28.88	36.11	32	0.00	0.00	97	28.88	36.11
97	57.33	71.66	32	28.44	35.55	33	+ 0.44	+ 0.55	98	29.33	36.66
96	56.88	71.11	31	28.00	35.00	34	0.88	1.11	99	29.77	37.22
95	56.44	70.55	30	27.55	34.44	35	1.33	1.66	100	30.22	37.77
94	56.00	70.00	29	27.11	33.88	36	1.77	2.22	101	30.66	38.33
93	55.55	69.44	28	26.66	33.33	37	2.22	2.77	102	31.11	38.88
92	55.11	68.88	27	26.22	32.77	38	2.66	3.33	103	31.55	39.44
91	54.66	68.33	26	25.77	32.22	39	3.11	3.88	104	32.00	40.00
90	54.22	67.77	25	25.33	31.66	40	3.55	4.44	105	32.44	40.55
89	53.77	67.22	24	24.88	31.11	41	4.00	5.00	106	32.88	41.11
88	53.33	66.66	23	24.44	30.55	42	4.44	5.55	107	33.33	41.66
87	52.88	66.11	22	24.00	30.00	43	4.88	6.11	108	33.77	42.22
86	52.44	65.55	21	23.55	29.44	44	5.33	6.66	109	34.22	42.77
85	52.00	65.00	20	23.11	28.88	45	5.77	7.22	110	34.66	43.33
84	51.55	64.44	19	22.66	28.33	46	6.22	7.77	111	35.11	43.88
83	51.11	63.88	18	22.22	27.77	47	6.66	8.33	112	35.55	44.44
82	50.66	63.33	17	21.77	27.22	48	7.11	8.88	113	36.00	45.00
81	50.22	62.77	16	21.33	26.66	49	7.55	9.44	114	36.44	45.55
80	49.77	62.22	15	20.88	26.11	50	8.00	10.00	115	36.88	46.11
79	49.33	61.66	14	20.44	25.55	51	8.44	10.55	116	37.33	46.66
78	48.88	61.11	13	20.00	25.00	52	8.88	11.11	117	37.77	47.22
77	48.44	60.55	12	19.55	24.44	53	9.33	11.66	118	38.22	47.77
76	48.00	60.00	11	19.11	23.88	54	9.77	12.22	119	38.66	48.33
75	47.55	59.44	10	18.66	23.33	55	10.22	12.77	120	39.11	48.88
74	47.11	58.88	9	18.22	22.77	56	10.66	13.33	121	39.55	49.44
73	46.66	58.33	8	17.77	22.22	57	11.11	13.88	122	40.00	50.00
72	46.22	57.77	7	17.33	21.66	58	11.55	14.44	123	40.44	50.55
71	45.77	57.22	6	16.88	21.11	59	12.00	15.00	124	40.88	51.11
70	45.33	56.66	5	16.44	20.55	60	12.44	15.55	125	41.33	51.66
69	44.88	56.11	4	16.00	20.00	61	12.88	16.11	126	41.77	52.22
68	44.44	55.55	3	15.55	19.44	62	13.33	16.66	127	42.22	52.77
67	44.00	55.00	2	15.11	18.88	63	13.77	17.22	128	42.66	53.33
66	43.55	54.44	—1	14.66	18.33	64	14.22	17.77	129	43.11	53.88
65	43.11	53.88	0	14.22	17.77	65	14.66	18.33	130	43.55	54.44
64	42.66	53.33	+1	13.77	17.22	66	15.11	18.88	131	44.00	55.00
63	42.22	52.77	2	13.33	16.66	67	15.55	19.44	132	44.44	55.55
62	41.77	52.22	3	12.88	16.11	68	16.00	20.00	133	44.88	56.11
61	41.33	51.66	4	12.44	15.55	69	16.44	20.55	134	45.33	56.66
60	40.88	51.11	5	12.00	15.00	70	16.88	21.11	135	45.77	57.22
59	40.44	50.55	6	11.55	14·44	71	17.33	21.66	136	46.22	57.77
58	40.00	50.00	7	11.11	13.88	72	17.77	22.22	137	46.66	58.33
57	39.55	49.44	8	10.66	13.33	73	18.22	22.77	138	47.11	58.88
56	39.11	48.88	9	10.22	12.77	74	18.66	23.33	139	47.55	59.44
55	38.66	48.33	10	9.77	12.22	75	19.11	23.88	140	48.00	60.00
54	38.22	47.77	11	9.33	11.66	76	19.55	24.44	141	48.44	60.55
53	37.77	47.22	12	8.88	11.11	77	20.00	25.00	142	48.88	61.11
52	37.33	46.66	13	8.44	10.55	78	20.44	25.55	143	49.33	61.66
51	36.88	46.11	14	8.00	10.00	79	20.88	26.11	144	49.77	62.22
50	36.44	45.55	15	7.55	9.44	80	21.33	26.66	145	50.22	62.77
49	36.00	45.00	16	7.11	8.88	81	21.77	27.22	146	50.66	63.33
48	35.55	44.44	17	6.66	8.33	82	22.22	27.77	147	51.11	63.88
47	35.11	43.88	18	6.22	7.77	83	22.66	28.33	148	51.55	64.44
46	34.66	43.33	19	5.77	7.22	84	23.11	28.88	149	52.00	65.00
45	34.22	42.77	20	5.33	6.66	85	23.55	29.44	150	52.44	65.55
44	33.77	42.22	21	4.88	6.11	86	24.00	30.00	151	52.88	66.11
43	33.33	41.66	22	4.44	5.55	87	24.44	30.55	152	53.33	66.66
42	32.88	41.11	23	4.00	5.00	88	24.88	31.11	153	53.77	67.22
41	32.44	40.55	24	3.55	4.44	89	25.33	31.66	154	54.22	67.77
40	32.00	40.00	25	3.11	3.88	90	25.77	32.22	155	54.66	68.33
39	31.55	39.44	26	2.66	3.33	91	26.22	32.77	156	55.11	68.88
38	31.11	38.88	27	2.22	2.77	92	26.66	33.33	157	55.55	69.44
37	30.66	38.33	28	1.77	2.22	93	27.11	33.88	158	56.00	70.00
—36	—30.22	—37.77	+29	—1.33	—1.66	+94	+27.55	+34.44	+159	+56.44	+70.5

TABLE III.—*continued.*

Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.	Cent.	Fahr.	Reaum.
+160	+56.88	+71.11	+225	+85.77	+107.22	+290	+114.66	+143.33	+355	+143.55
161	57.33	71.66	226	86.22	107.77	291	115.11	143.88	356	144.00
162	57.77	72.22	227	86.66	108.33	292	115.55	144.44	357	144.44
163	58.22	72.77	228	87.11	108.88	293	116.00	145.00	358	144.88
164	58.66	73.33	229	87.55	109.44	294	116.44	145.55	359	145.33
165	59.11	73.88	230	88.00	110.00	295	116.88	146.11	360	145.77
166	59.55	74.44	231	88.44	110.55	296	117.33	146.66	361	146.22
167	60.00	75.00	232	88.88	111.11	297	117.77	147.22	362	146.66
168	60.44	75.55	233	89.33	111.66	298	118.22	147.77	363	147.11
169	60.88	76.11	234	89.77	112.22	299	118.66	148.33	364	147.55
170	61.33	76.66	235	90.22	112.77	300	119.11	148.88	365	148.00
171	61.77	77.22	236	90.66	113.33	301	119.55	149.44	366	148.44
172	62.22	77.77	237	91.11	113.88	302	120.00	150.00	367	148.88
173	62.66	78.33	238	91.55	114.44	303	120.44	150.55	368	149.33
174	63.11	78.88	239	92.00	115.00	304	120.88	151.11	369	149.77
175	63.55	79.44	240	92.44	115.55	305	121.33	151.66	370	150.22
176	64.00	80.00	241	92.88	116.11	306	121.77	152.22	371	150.66
177	64.44	80.55	242	93.33	116.66	307	122.22	152.77	372	151.11
178	64.88	81.11	243	93.77	117.22	308	122.66	153.33	373	151.55
179	65.33	81.66	244	94.22	117.77	309	123.11	153.88	374	152.00
180	65.77	82.22	245	94.66	118.33	310	123.55	154.44	375	152.44
181	66.22	82.77	246	95.11	118.88	311	124.00	155.00	376	152.88
182	66.66	83.33	247	95.55	119.44	312	124.44	155.55	377	153.33
183	67.11	83.88	248	96.00	120.00	313	124.88	156.11	378	153.77
184	67.55	84.44	249	96.44	120.55	314	125.33	156.66	379	154.22
185	68.00	85.00	250	96.88	121.11	315	125.77	157.22	380	154.66
186	68.44	85.55	251	97.33	121.66	316	126.22	157.77	381	155.11
187	68.88	86.11	252	97.77	122.22	317	126.66	158.33	382	155.55
188	69.33	86.66	253	98.22	122.77	318	127.11	158.88	383	156.00
189	69.77	87.22	254	98.66	123.33	319	127.55	159.44	384	156.44
190	70.22	87.77	255	99.11	123.88	320	128.00	160.00	385	156.88
191	70.66	88.33	256	99.55	124.44	321	128.44	160.55	386	157.33
192	71.11	88.88	257	100.00	125.00	322	128.88	161.11	387	157.77
193	71.55	89.44	258	100.44	125.55	323	129.33	161.66	388	158.22
194	72.00	90.00	259	100.88	126.11	324	129.77	162.22	389	158.66
195	72.44	90.55	260	101.33	126.66	325	130.22	162.77	390	159.11
196	72.88	91.11	261	101.77	127.22	326	130.66	163.33	391	159.55
197	73.33	91.66	262	102.22	127.77	327	131.11	163.88	392	160.00
198	73.77	92.22	263	102.66	128.33	328	131.55	164.44	393	160.44
199	74.22	92.77	264	103.11	128.88	329	132.00	165.00	394	160.88
200	74.66	93.33	265	103.55	129.44	330	132.44	165.55	395	161.33
201	75.11	93.88	266	104.00	130.00	331	132.88	166.11	396	161.77
202	75.55	94.44	267	104.44	130.55	332	133.33	166.66	397	162.22
203	76.00	95.00	268	104.88	131.11	333	133.77	167.22	398	162.66
204	76.44	95.55	269	105.33	131.66	334	134.22	167.77	399	163.11
205	76.88	96.11	270	105.77	132.22	335	134.66	168.33	+400	+163.55
206	77.33	96.66	271	106.22	132.77	336	135.11	168.88		
207	77.77	97.22	272	106.66	133.33	337	135.55	169.44		
208	78.22	97.77	273	107.11	133.88	338	136.00	170.00		
209	78.66	98.33	274	107.55	134.44	339	136.44	170.55		
210	79.11	98.88	275	108.00	135.00	340	136.88	171.11		
211	79.55	99.44	276	108.44	135.55	341	137.33	171.66		
212	80.00	100.00	277	108.88	136.11	342	137.77	172.22		
213	80.44	100.55	278	109.33	136.66	343	138.22	172.77		
214	80.88	101.11	279	109.77	137.22	344	138.66	173.33		
215	81.33	101.66	280	110.22	137.77	345	139.11	173.88		
216	81.77	102.22	281	110.66	138.33	346	139.55	174.44		
217	82.22	102.77	282	111.11	138.88	347	140.00	175.00		
218	82.66	103.33	283	111.55	139.44	348	140.44	175.55		
219	83.11	103.88	284	112.00	140.00	349	140.88	176.11		
220	83.55	104.44	285	112.44	140.55	350	141.33	176.66		
221	84.00	105.00	286	112.88	141.11	351	141.77	177.22		
222	84.44	105.55	287	113.33	141.66	352	142.22	177.77		
223	84.88	106.11	288	113.77	142.22	353	142.66	178.33		
224	+85.33	+106.66	+289	+114.22	+142.77	+354	+143.11	+178.88		

PROPORTIONAL P

Fahr.	Reaum.
0.1	0.044
0.2	0.088
0.3	0.133
0.4	0.177
0.5	0.222
0.6	0.266
0.7	0.311
0.8	0.355
0.9	0.400

TABLE IV.

im.	French		French Lines.	English inches.
	In.	Lines.		
20	26	7.17	319.17	28.347
21		7.62	319.62	28.386
22		8.06	320.06	28.426
23		8.50	320.50	28.465
24		8.95	320.95	28.504
25		9.39	321.39	28.544
26		9.83	321.83	28.583
27		10.28	322.28	28.623
28		10.72	322.72	28.662
29		11.16	323.16	28.701
30	26	11.61	323.61	28.741
31	27	0.05	324.05	28.780
32		0.49	324.49	28.819
33		0.94	324.94	28.859
34		1.38	325.38	28.898
35		1.82	325.82	28.937
36		2.27	326.27	28.977
37		2.71	326.71	29.016
38		3.15	327.15	29.056
39		3.60	327.60	29.095
740		4.04	328.04	29.134
741		4.48	328.48	29.174
742		4.93	328.93	29.213
743		5.37	329.37	29.252
744		5.81	329.81	29.292
745		6.26	330.26	29.331
746		6.70	330.70	29.371
747		7.14	331.14	29.410
748		7.58	331.58	29.449
749		8.03	332.03	29.489
750		8.47	332.47	29.528
751		8.91	332.91	29.567
752		9.36	333.36	29.607
753		9.80	333.80	29.646
754		10.24	334.24	29.686
755		10.69	334.69	29.725
756		11.13	335.13	29.764
757	27	11.57	335.57	29.804
758	28	0.02	336.02	29.843

Millim.	French		French Lines.	English inches.
	In.	Lines.		
759	28	0.46	336.46	29.882
760		0.90	336.90	29.922
761		1.35	337.35	29 961
762		1.79	337.79	30.000
763		2.23	338.23	30.040
764		2.68	338.68	30.079
765		3.12	339.12	30.119
766		3.56	339.56	30.158
767		4.01	340.01	30.197
768		4.45	340.45	30.237
769		4.89	340.89	30.276
770		5.34	341.34	30.315
771		5.78	341.78	30.355
772		6.22	342.22	30.394
773		6.67	342.67	30.434
774		7.11	343.11	30.473
775		7.55	343.55	30.512
776		8.00	344.00	30.552
777		8.44	344.44	30.591
778		8.88	344.88	30.630
779		9.33	345.33	30.670
780	28	9.77	345.77	30.709

PROPORTIONAL PARTS.

Millim.	French lines.	English inches.
0.1	0.045	0.0039
0.2	0.090	.0078
0.3	0.135	.0118
0.4	0.180	.0157
0.5	0.225	.0196
0.6	0.270	.0236
0.7	0.315	.0275
0.8	0.360	.0314
0.9	0.405	.0354
1.0	0.450	0.0393

TABLE V.

ench		Fr. Lin.	English inches.	Millim.	French		Fr. Lin.	English inches.	Millim.
n.	Lin.				In.	Lin.			
26	1	313	27.799	706.128	27	7	331	29.397	746.736
	2	314	27.887	708.384		8	332	29.486	748.992
	3	315	27.976	710.640		9	333	29.575	751.248
	4	316	28.065	712.896		10	334	29.664	753.504
	5	317	28.154	715.152	27	11	335	29.753	755.760
	6	318	28.243	717.408	28	0	336	29.841	758.016
	7	319	28.332	719.664		1	337	29.930	760.292
	8	320	28.420	721.920		2	338	30.019	762.528
	9	321	28.509	724.176		3	339	30.108	764.784
	10	322	28.598	726.432		4	340	30.197	767.040
26	11	323	28.687	728.688		5	341	30.285	769.296
7	0	324	28.776	730.944		6	342	30.374	771.552
	1	325	28.864	733.200		7	343	30.463	773.808
	2	326	28.953	735.456		8	344	30.552	776.064
	3	327	29.042	737.712		9	345	30.641	778.320
	4	328	29.131	739.968		10	346	30.730	780.576
	5	329	29.220	742.224	28	11	347	30.818	782.832
	6	330	29.309	744.480	29	0	348	30.907	785.088

PROPORTIONAL PARTS.

French Lines.	English inches.	Millim.
0.1	0.0089	0.2256
0.2	0.0178	0.4512
0.3	0.0266	0.6768
0.4	0.0355	0.9024
0.5	0.0444	1.1280
0.6	0.0533	1.3536
0.7	0.0622	1.5792
0.8	0.0710	1.8048
0.9	0.0799	2.0304
1.0	0.0888	2.2560

TABLE VI.

English inches.	French		French Lines.	Millim.
	In.	Lines.		
27.1	25	5.12	305.12	688.31
27.2		6.25	306.25	690.85
27.3		7.38	307.38	693.39
27.4		8.50	308.50	695.93
27.5		9.63	309.63	698.47
27.6		10.75	310.75	701.01
27.7	25	11.88	311.88	703.55
27.8	26	1.01	313.01	706.09
27.9		2.13	314.13	708.63
28.0		3.26	315.26	711.17
28.1		4.38	316.38	713.71
28.2		5.51	317.51	716.25
28.3		6.64	318.64	718.79
28.4		7.76	319.76	721.33
28.5		8.89	320.89	723.87
28.6		10.01	322.01	726.41
28.7	26	11.14	323.14	728.95
28.8	27	0.27	324.27	731.49
28.9		1.39	325.39	734.03
29.0		2.52	326.52	736.57
29.1		3.64	327.64	739.11
29.2		4.77	328.77	741.65
29.3		5.90	329.90	744.19
29.4		7.02	331.02	746.73
29.5		8.14	332.14	749.27
29.6		9.27	333.27	751.81
29.7		10.40	334.40	754.35
29.8	27	11.52	335.52	756.89
29.9	28	0.65	336.65	759.43
30.0		1.78	337.78	761.97
30.1		2.90	338.90	764.51
30.2		4.03	340.03	767.05
30.3		5.15	341.15	769.59
30.4		6.28	342.28	772.13
30.5		7.41	343.41	774.67
30.6		8.53	344.53	777.21
30.7		9.66	345.66	779.75
30.8		10.78	346.78	782.29
30.9	28	11.91	347.91	784.83
31.0	29	1.04	349.04	787.37

PROPORTIONAL PARTS.

English In.	Fr. Lines.	Millim.
0.01	0.1126	0.254
0.02	0.2252	0.508
0.03	0.3378	0.762
0.04	0.4504	1.016
0.05	0.5630	1.270
0.06	0.6755	1.524
0.07	0.7881	1.778
0.08	0.9007	2.032
0.09	1.0133	2.286
0.10	1.1259	2.5399

TABLE VII.—TOISES.

Toises.	Metres.	English Feet.	Toises.	Metres.	English Feet.
1	1.94904	6.39459	200	389.80726	1278.91832
2	3.89807	12.78918	300	584.71089	1918.37748
3	5.84711	19.18377	400	779.61452	2557.83664
4	7.79615	25.57837	500	974.51815	3197.29580
5	9.74518	31.97296	600	1169.42179	3836.75496
6	11.69422	38.36755	700	1364.32542	4476.21412
7	13.64325	44.76214	800	1559.22905	5115.67328
8	15.59229	51.15673	900	1754.13268	5755.13244
9	17.54133	57.55132	1000	1949.03631	6394.59160
10	19.49036	63.94592	2000	3898.07262	12789.18321
20	38.98073	127.89183	3000	5847.10893	19183.77481
30	58.47109	191.83775	4000	7796.14524	25578.36642
40	77.96145	225.78366	5000	9745.18155	31972.95802
50	97.45182	319.72958	6000	11694.21786	38367.54963
60	116.94218	383.67550	7000	13643.25417	44762.14123
70	136.43254	447.62141	8000	15592.29084	51156.73284
80	155.92290	511.56733	9000	17541.32679	57551.32444
90	175.41327	575.51324	10000	19490.36310	63945.91605
100	194.90363	639.45916			

TABLE VIII.—French Feet.

ch t.	Toises.	Metres.	English Feet and Inch.		French Feet.	Toises.	Metres.	English Feet and Inch.	
			Feet.	Inch.				Feet.	Inch.
	0.16667	0.32484	1	0.7892	200	33.33333	64.96788	213	1.8366
	0.33333	0.64968	2	1.5784	300	50.00000	97.45182	319	8.7550
	0.50000	0.97452	3	2.3675	400	66.66667	129.93575	426	3.6733
	0.66667	1.29936	4	3.1567	500	83.33333	162.41969	532	10.5916
	0.83333	1.62420	5	3.9459	600	100.00000	194.90363	639	5.5099
	1.00000	1.94904	6	4.7351	700	116.66667	227.38757	746	0.4282
	1.16667	2.27388	7	5.5243	800	133.33333	259.87151	852	7.3466
	1.33333	2.59872	8	6.3135	900	150.00000	292.35545	959	2.2649
	1.50000	2.92355	9	7.1026	1000	166.66667	324.83938	1065	9.1832
	1.66667	3.24839	10	7.8918	2000	333.33333	649.67877	2131	6.3664
	3.33333	6.49679	21	3.7837	3000	500.00000	974.51815	3197	3.5496
	5.00000	9.74518	31	11.6755	4000	666.66667	1299.35754	4263	0.7328
	6.66667	12.99358	42	7.5673	5000	833.33333	1624.19692	5328	9.9160
	8.33333	16.24197	53	3.4592	6000	1000.00000	1949.03631	6394	7.0993
	10.00000	19.49036	63	11.3510	7000	1166.66667	2273.87569	7460	4.2825
	11.66667	22.73876	74	7.2428	8000	1333.33333	2598.71508	8526	1.4657
	13.33333	25.98715	85	3.1347	9000	1500.00000	2923.55446	9591	10.6489
	15.00000	29.23554	95	11.0265	10000	1666.66667	3248.39385	10657	7.8321
	16.66667	32.48394	106	6.9183					

ABLE IX.—French Inches.

nch hes.	Toises.	Millimeters.	English Inches.
1	0.01389	27.070	1.0658
2	0.02778	54.140	2.1315
3	0.04167	81.210	3.1973
4	0.05556	108.280	4.2631
5	0.06944	135.350	5.3288
6	0.08333	162.420	6.3946
7	0.09722	189.490	7.4604
8	0.11111	216.560	8.5261
9	0.12500	243.630	9.5919
10	0.13889	270.699	10.6577
11	0.15278	297.769	11.7234
12	0.16667	324.839	12.7899

TABLE X.—French Lines.

French Lines.	Toises.	Millimeters.	English Inches.
1	0.00116	2.256	0.0888
2	0.00231	4.512	0.1776
3	0.00347	6.767	0.2664
4	0.00463	9.023	0.3553
5	0.00579	11.279	0.4441
6	0.00694	13.535	0.5329
7	0.00810	15.791	0.6217
8	0.00926	18.046	0.7105
9	0.01042	20.302	0.7993
10	0.01157	22.558	0.8881
11	0.01273	24.814	0.9770
12	0.01389	27.070	1.0658

TABLE XI.—Metres.

t.	Toises.	French			English		Met.	Toises.	French			English	
		Feet.	In.	Lines.	Feet.	Inch.			Feet.	In.	Lines.	Feet.	Inch.
	0.51307	3	0	11.296	3	3.3708	200	102.61481	615	8	3.200	656	2.1580
	1.02615	6	1	10.592	6	6.7416	300	153.92222	923	6	4.800	984	3.2370
	1.53922	9	2	9.888	9	10.1124	400	205.22963	1231	4	6.400	1312	4.3160
	2.05230	12	3	9.184	13	1.4832	500	256.53704	1539	2	8.000	1640	5.3950
	2.56537	15	4	8.480	16	4.8539	600	307.84444	1847	0	9.600	1968	6.4740
	3.07844	18	5	7.776	19	8.2247	700	359.15185	2154	10	11.200	2296	7.5530
	3.59152	21	6	7.072	22	11.5955	800	410.45926	2462	9	0.800	2624	8.6320
	4.10459	24	7	6.368	26	2.9663	900	461.76667	2770	7	2.400	2952	9.7110
	4.61767	27	8	5.664	29	6.3371	1000	513.07407	3078	5	4.000	3280	10.7900
	5.13074	30	9	4.960	32	9.7079	2000	1026.14815	6156	10	8.000	6561	9.5800
	10.26148	61	6	9.920	65	7.4158	3000	1539.22222	9235	4	0.000	9842	8.3700
	15.39222	92	4	2.880	98	5.1237	4000	2052.29630	12313	9	4.000	13123	7.1600
	20.52296	123	1	7.840	131	2.8316	5000	2565.37037	15392	2	8.000	16404	5.9500
	25.65370	153	11	0.800	164	0.5395	6000	3078.44444	18470	8	0.000	19685	4.7400
	30.78444	184	8	5.760	196	10.2474	7000	3591.51852	21549	1	4.000	22066	3.5300
	35.91519	215	5	10.720	229	7.9553	8000	4104.59259	24627	6	8.000	26247	2.3200
	41.04593	246	3	3.680	262	5.6632	9000	4617.66667	27706	0	0.000	29528	1.1100
	46.17667	277	0	8.640	295	3.3711	10000	5130.74074	30784	5	4.000	32808	11.9000
	51.30741	307	10	1.600	328	1.0790							

TABLE XII.—MILLIMETRES.

Millim.	Toises.	French lines.	English lines.	Millim.	Toises.	French lines.	English line
1	0.00051	0.443	0.0394	60	0.03078	26.598	2.3622
2	0.00103	0.887	0.0787	70	0.03592	31.031	2.7560
3	0.00154	1.330	0.1181	80	0.04105	35.464	3.1497
4	0.00205	1.773	0.1575	90	0.04618	39.897	3.5434
5	0.00257	2.216	0.1969	100	0.05131	44.330	3.9371
6	0.00308	2.660	0.2362	200	0.10261	88.659	7.8742
7	0.00359	3.103	0.2756	300	0.15392	132.989	11.8112
8	0.00410	3.546	0.3150	400	0.20523	177.318	15.7483
9	0.00462	3.990	0.3543	500	0.25654	221.648	19.6854
10	0.00513	4.433	0.3937	600	0.30784	265.978	23.6225
20	0.01026	8.866	0.7874	700	0.35915	310.307	27.5596
30	0.01539	13.299	1.1811	800	0.41046	354.637	31.4966
40	0.02052	17.732	1.5748	900	0.46177	398.966	35.4337
50	0.02565	22.165	1.9685				

TABLE XIII.—ENGLISH FEET.

Eng. feet.	Toises.	Metres.	French			English feet.	Toises.	Metres.	French		
			Feet.	In.	Lines.				Feet.	In.	Line
1	0.15638	0.30479	0	11	3.114	200	31.27643	60.95850	187	7	10.83
2	0.31276	0.60959	1	10	6.228	300	46.91465	91.43835	281	5	10.25
3	0.46915	0.91438	2	9	9.343	400	62.55286	121.91780	375	3	9.67
4	0.62553	1.21918	3	9	0.457	500	78.19108	152.39725	469	1	9.09
5	0.78191	1.52397	4	8	3.571	600	93.82929	182.87670	562	11	8.50
6	0.93829	1.82877	5	7	6.685	700	109.46751	213.35615	656	9	7.92
7	1.09468	2.13356	6	6	9.799	800	125.10572	243.83559	750	7	7·34
8	1.25106	2.43836	7	6	0.913	900	140.74394	274.31504	844	5	6.76
9	1.40744	2.74315	8	5	4.028	1000	156.38215	304.79449	938	3	6·18
10	1.56382	3.04794	9	4	7.142	2000	312.76431	609.58899	1876	7	0·36
20	3.12764	6.09589	18	9	2.284	3000	469.14646	914.38348	2814	10	6·53
30	4.69146	9.14383	28	1	9.425	4000	625.52861	1219.17797	3753	2	0·71
40	6.25529	12.19178	37	6	4.567	5000	781.91076	1523.97246	4691	5	6·89
50	7.81911	15.23972	46	10	11.709	6000	938.29292	1828.76696	5629	9	1·07
60	9.38293	18.28767	56	3	6.851	7000	1094.67507	2133.56145	6568	0	7·25
70	10.94675	21.33561	65	8	1.993	8000	1251.05722	2438.35594	7506	4	1·43
80	12.51057	24.38536	75	0	9.134	9000	1407.43937	2743.15044	8444	7	7·61
90	14.07439	27.43150	84	5	4.276	10000	1563.82153	3047.94493	9382	11	1.79
100	15.63822	30.47945	93	9	11.418						

TABLE XIV.—ENGLISH INCHES.

Inch.	Toises.	French		Millim.	Inch.	Toises.	French		Millim.
		In.	Lines.				In.	Lines.	
1	0.01303	0	11.260	25.400	7	0.09122	6	6.817	177.797
2	0.02606	1	10.519	50.799	8	0.10426	7	6.076	203.197
3	0.03910	2	9.779	76.199	9	0.11729	8	5.336	228.596
4	0.05213	3	9.038	101.598	10	0.13032	9	4.595	253.995
5	0.06516	4	8.298	126.998	11	0.14335	10	3.855	279.395
6	0.07819	5	7.557	152.397					

On Two Attempts to ascend Chimborazo. By ALEXANDER VON HUMBOLDT. Translated from the German, and communicated, at the request of the Author, by Dr MARTIN BARRY.

THE highest mountain-summits of both continents,—in the old continent, Dhawalagiri (White Mountain) and the Jawahir; in the new, the Sorata and the Illimani,—remain unreached by man. The highest point of the earth's surface attained, lies in South America on the south-east side of Chimborazo. There travellers have reached the height of nearly 18,500 Paris feet[*] —viz. in June 1802, 3016 toises,[†] in December 1831, 3080 toises, above the level of the sea. Barometrical measurements have thus been made, in the chain of the Andes 3720 (Paris) feet above the level of the summit of Mont Blanc. The height of Mont Blanc is in relation to that of the Cordilleras so inconsiderable, that in the latter, there are much frequented passes that are higher; indeed, the upper part of the great city of Potosi has an elevation only 323 toises inferior to that of the summit of Mont Blanc. I have thought it needful to premise these numerical statements, in order to present to the imagination definite points of comparison for the hypsometric, as it were plastic, contemplation of the surface of the earth.

The attainment of great heights is of less scientific interest, when the same lie far above the snow-line, and can be visited for a few hours only. Immediate barometrical measurements of heights afford indeed this advantage, that the results are quickly obtained, yet the summits are, for the most part, surrounded by high plains, adapted for trigonometrical operations, by which all the elements of the measurements can be repeatedly proved; whilst a single determination by means of the barometer, is liable to considerable errors, because of the ascending and descending currents of air on the mountain slopes, and the variation in the decrease of temperature thus occasioned. The nature of the rocks, from the permanent covering of snow, is almost entirely withdrawn from geognostic observation, since there are presented only single ridges composed of much weathered strata. Organic life ceases in these lofty solitudes. Scarcely do the condor and winged insects stray into these attenuated strata of the atmosphere, the latter being carried up by the currents of air. If the endeavours of travelling natural philosophers, who strive to climb the higher summits of the earth, is scarcely rewarded by a serious scientific interest, there is, on the other hand, an active popular participation in such endeavours. That which seems unattainable has a mysterious attractive power; we wish that all should be explored,—at least attempted, though not to be obtained. Chimborazo has been the wearisome object of all inquiries addressed to me since my first return to Europe. The thoroughly exploring of the most important laws of nature, the most vivid delineations of stratified zones of plants and differences in climates, determining, as the latter do, the object of agriculture, were seldom capable of diverting attention from the snow-clad summit which at that time (before Pentland's journey to Bolivia) was supposed to be the culminating point of the dike-like Andes.

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[*] One French foot is = 1.07892, or about $1\frac{1}{13}$ English.—Tr.

[†] A toise is = 1.94904 metres, or 6.39459 English feet.—Tr.

I shall here extract from the still unprinted portion of my journals, the simple narration of a mountain journey. The entire detail of the trigonometrical measurement, which I made at New Riobamba in the plain of Tapia, was made known in the introduction to the first volume of my *Astronomical Observations*, soon after my return. The geographical distribution of the plants on the acclivity of Chimborazo and the neighbouring mountains (from the sea coast up to a height of 14,800 feet), I have attempted to represent, by a figure in a table of my *Geographical and Physical Atlas of South America*, according to the excellent determination by Kunth of the alpine vegetation of the Cordilleras, collected by Bonpland and myself.

The history of the ascent itself, which can present but little dramatic interest, was reserved for the fourth and last volume of my journey towards the equatorial regions. But since my friend M. Boussingault, now Professor of Chemistry at Lyons, one of the most talented and learned travellers of modern times, has recently, at my request, described in the Annals de Chimie et de Physiques,* this enterprize, which very closely resembles my own,—and since our observations are mutually confirmative of each other,—this small fragment of a journey, which I here lay before the public, will no doubt be favoured with an indulgent reception. I shall provisionally refrain from all circumstantial geognostic and physical discussions.

On the 22d June 1799, I was in the crater of the Peak of Teneriffe. Three years afterwards, almost on the same day (the 23d June 1802), I reached a point 6700 feet higher, near the summit of Chimborazo. After a long delay in the tableland of Quito, one of the most wonderful and picturesque regions of the earth, we undertook the journey towards the forests of the Peruvian bark trees of Loxa, the upper course of the river Amazons, westward of the celebrated strait (Pongo de Manseriche), and through the sandy desert along the Peruvian coast of the South Sea towards Lima, where we were to observe the transit of Mercury on the 9th November 1802. On a plain covered with pumice-stone,—where (after the fearful earthquake of 4th February 1797) the building of the new city Riobamba was begun,—we enjoyed for several days a splendid view of the bell or dome-shaped summit of Chimborazo. We had the clearest weather, favouring trigonometrical observation. By means of a large telescope, we had thoroughly examined the snow-mantle of the mountain, still 15,700 toises distant, and discovered several ridges, which, projecting like sterile black streaks, converged towards the summit, and gave some hope that, upon them, a firm footing might be obtained in the region of eternal snow. Riobamba Nuevo lies within sight of the enormous and now indented mountain Capac-urcu, called by the Spaniards El Altar, which (says a tradition of the natives) was once higher than Chimborazo, and after having been many years in a state of eruption, suddenly fell in. This terror-spreading event is said to have taken place shortly before the conquest of Quito by the Inca Tupac-Yupanqui. Riobamba Nuevo must not be confounded with the old Riobamba of the great map of La Condamine and Don Pedro Maldonado. The latter city was entirely destroyed by the great catastrophe of the 4th February 1797, which in a few minutes destroyed 45,000 human beings. The new Riobamba lies, according to my chronometrical observations, 42 seconds more to the eastward than the old Riobamba, but almost in the same latitude (1° 41′ 46″ south). We were in the plain of Tapia, from which, on the 22d June, we began our expedition towards Chimborazo, being already 8898 Paris feet[*] (1483 toises) above the level of the South Sea. This table-land is a part of the valley-land between the Eastern and Western Andes, *i. e.* between the chain of the active volcanoes Cotopaxi and Tungurahua on the one hand, and the chain of the Iliniza and Chimborazo on the other. We gently ascended as far as the foot of the last mentioned mountain, where, in the Indian village Calpi, we were to pass the night. This plain is sparingly covered with Cactus stems and Schinus molle, which resembles a weeping willow. Herds of variegated llamas, in thousands, seek here a scanty subsistence. At so great a height, the nightly terrestrial radiation of heat, when the sky is cloudless, proves injurious to agriculture, through cold and frost. Before reaching Calpi, we visited Lican, now likewise a small village, but before the conquest of the country by the eleventh Inca,[†] a considerable city, and the place of residence of the Conchocando, or Prince of Puruay. The natives believe that the few wild llamas found on the western side of Chimborazo, are derived from dispersed and fugitive herds, which, after the destruction of the old Lican, became wild.

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* See Edinburgh New Philosophical Journal, vol. xix. for 1835, where there is a translation of Boussingault's memoir.

Very near to Calpi, north-westward of Lican, there is in the barren table-land a little isolated hill, the *black mountain*, Yana-Urcu, the name of which has not been given by the French academicians, but which, in a geognostical point of view, deserves much attention. The hill lies SS.E. of Chimborazo, at a distance of less than three miles (15 to 1°), and separated from the same by the high plain of Luisa only. If in it we do not recognise a lateral eruption of Chimborazo, the origin of the cone must certainly be ascribed to the subterranean forces which, under that mountain, have for thousands of years vainly sought an opening. It is of later origin than the elevation of the great dome-shaped mountain. The Yana-Urcu forms, with the northern hill Naguangachi, a connected eminence in the form of a horse-shoe; the bow, more than a semicircle, is open towards the east. There probably lies in the centre of the horse-shoe, the point out of which the black slags have been thrown, that now lie spread far around. We found there a funnel-shaped depression of about 120 feet in depth, in the interior of which there is a small hill, whose height does not equal that of the surrounding margin. Yana-Urcu probably signifies the southern culminating point of the old crater-margin, which, at the most, is elevated 400 feet above the level of Calpi. Naguangachi signifies the northern lower end. The whole eminence reminds one,—through its horse-shoe form, not in regard to its rock,—of the somewhat higher hill Javirac (el Panecillo de Quito), which rises isolated at the foot of the volcano Pichincha, in the plain of Turubamba, and which, in La Condamine's, or rather Morainville's map, is drawn erroneously as a perfect cone. According to the tradition of the natives, and according to old MSS. which the Cacike or Apu of Lican (the Conchocandi) possessed, the volcanic eruption of the Yana-Urca occurred immediately after the death of the Inca Tupa-Yupanqui:—thus probably in the middle of the 15th century. Tradition says that a fire-ball, or indeed a star, fell from heaven and set on fire the mountain. Such fables, connecting the fall of aërolites with eruptions are also spread among the tribes of Mexico. The rock of the Yana-Urcu is a porous dark clove-coloured, often entirely black slaggy mass, which may be easily confounded with porous basalt. Olivin is entirely wanting in it, the white, sparingly distributed crystals it contains, are throughout small and probably Labrador. Here and there, I saw a sprinkling of iron-pyrites. The whole belongs probably to the black augite-porphyry, as well as the whole formation of Chimborazo, of which we shall speak further on, and to which I am not disposed to give the name trachyte, since it contains no felspar, (with some albite), such as is contained in our trachyte of the Siebengebirge near

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[*] Thus 2890 metres. Boussingault calculated this elevation to be 2870 metres, and estimated the mean temperature of the high plain of Tabia at 16°.4 C. (61°52 F.)

[†] The same Tupac-Yupanqui, whose well preserved remains, Garcillasso de la Vega, so lately as in 1559, had seen in the family-vault at Cuzco.

Bonn. The slaggy masses of the Yana-Urcu altered by a very active fire, are indeed extremely light, but proper pumice-stone has not been thrown out there. The eruption has taken place through a grey, irregularly stratified mass of dolerite, which here forms the table-land, and resembles the rock of Penipe (at the foot of the volcano of Tungurahua) where syenite and mica-slate containing garnets, have been broken through. On the eastern side of the Yana-Urcu, or rather at the foot of the hill towards Lican, the natives conducted us to a projecting rock, an opening in which resembled the mouth of a forsaken gallery. Here, as well as at the distance of ten feet, there is heard a violent subterranean noise, which is accompanied by a current of air, or subterranean wind. The current of air is much too weak to admit of the noise being attributed to it. The noise certainly arises from a subterranean brook, which is precipitated downwards into a deep hollow, and through its fall occasions a motion in the air. A monk, the priest at Calpi, had, with the same idea, some time before, continued on the gallery at an open fissure to procure water for his village. The hardness of the black augite rock probably interrupted the work. Chimborazo, notwithstanding its enormous mass of snow, sends down into the table-land such insignificant brooks of water, that it may be presumed the greater part of its water flows through clefts to the interior. In the village of Calpi itself also, there was formerly heard a great noise in a house that had no cellar. Before the celebrated earthquake of the 4th February 1797, there sprang forth a brook in the south-west of the village, at a deeper point. Many Indians considered this brook as a part of the water that flows under the Yana-Urcu. But since the great earthquake, this brook has again disappeared.

After we had passed the night at Calpi, which, according to my barometrical measurement, lies 9720 feet (1620 toises) above the sea, we began, on the morning of the 23d, our proper expedition up Chimborazo. We attempted to ascend the mountain on the SS. E. side, and the Indians who were to attend us as guides, but of whom but a few had ever reached the limit of perpetual snow, gave this course the preference. We found Chimborazo surrounded with great plains, which rise, step-like, one above the other. Proceeding first through the Llanos de Luisa, then after rather a gradual ascent of scarcely 5000 feet in length, we reached the table land (Llano) of Sisgun. The first step (stufe) is at a height of 10,200 feet, the second 11,700. These grass grown plains thus equal in elevation, respectively, the highest summit of the Pyrenees (Peak Nethou) and the summit of the Peak of Teneriffe. The perfect horizontality of these table-lands allows us to infer the long continuance of stagnant water. The traveller imagines he sees before him the bottom of a lake. On the acclivity of the Swiss Alps, there is sometimes observed this phenomenon of small step-like plains, lying one above the other, which, like the emptied basins of alpine lakes, are united by narrow open passes. The widely extended grass lands (los Pajonales) are on Chimborazo, as everywhere around the high summits of the Andes, so monotonous that the family of the grasses (species of Paspalum, Andropogon, Bromus, Dejeuxia, Stipa) are seldom interrupted by dicotyledonous plants. There prevails almost the heathy scenery which I have seen in the barren part of Northern Asia. The flora of Chimborazo, in general, appeared to us less rich than that of the other snow mountains which surround the city of Quito. But a few Calceolariæ, Compositæ, (Bidens, Eupatorium, Dumerilia paniculata, Werneria nubigena) and Gentianæ, among which the beautiful Gentiana cernua shining forth with purple flowers,—rear themselves on the high plain of Sisgun, between the associated grasses. These belong, for the most part, to the genera of Northern Europe. The temperature of the air generally prevailing in these regions of alpine grasses, elevated respectively 1600 and 2000 toises, fluctuates by day between 4° and 16° C. (39°.2 and 60°.8 F.), by night between 0° and 10° (32° and 50° F.). The mean temperature of the whole year, according to my collective observations in the neighbourhood of the Equator, appears to be about 9° * (48°.2 F.). In the flat lands of the Temperate Zone, this is the mean temperature of the north of Germany,

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* All temperatures mentioned in this paper are expressed in degrees of the centigrade thermometer. (The equivalent degree of Fahrenheit have been since added.—Tr.)

for example, of Luneburg (Lat. 53°15′); but here the distribution of heat among the different months (the most important element in determining the character of the vegetation of a country) is so unequal, that in February, the mean heat is — 1°.8 (+28°.76 F.), in July + 18° (+ 64°.4 F.)

My plan was to perform a trigonometrical operation in the beautiful perfectly level grass land of Sisgun. I had made arrangements for measuring a base line here. The angles of altitude would have proved very considerable in such proximity to the summit of Chimborazo. There remained yet a perpendicular height of less than 8400 feet (the height of the Canigou in the Pyrenees) to determine. Yet with the enormous masses of single mountains in the chain of the Andes, every determination of the height above the sea is compounded of a barometrical and trigonometrical observation. I had taken with me the sextant and other instruments of measurement in vain. The summit of Chimborazo remained densely veiled in mist. From the high plain of Sisgun the ascent is tolerably steep as far as the little alpine lake of Yana-Coche. Thus far I had remained on the mule, having from time to time alighted with my travelling companion M. Bonpland merely to collect plants. Yana-Coche does not deserve the name of a lake. It is a circular basin of scarcely 130 feet in diameter. The sky became more and more obscured; but between and over the mist-strata there still lay scattered single groups of clouds. The summit of Chimborazo was visible for a few moments only at a time. Much snow having fallen during the preceding night, I left the mule where we found the lower border of this newly-fallen snow, a border which must not be confounded with the limit of perpetual snow. The barometer shewed that we had only now attained the height of 13,500 feet. On other mountains, likewise near to the equator, I have seen snow fall at the height of 11,200 feet, but not lower. My companion rode as far as the line of perpetual snow, *i. e.* to the height of Mont Blanc; which mountain, as is known, would not in this latitude (1° 27′ south) always be covered with snow. The horses and mules remained there to await our return.

A hundred and fifty toises above the little basin of Yanache, we saw at length naked rock. Hitherto the grass-land had withdrawn the ground from any geognostical examination. Great walls of rocks, extending from the N. E. towards the S. W., in part cleft into misshapen columns, reared themselves out of the eternal snow,—a brownish-black augite rock shining like pitch-stone porphyry. The columns were very thin, perhaps fifty to sixty feet in height, almost like the trachyte columns of Tabla-Umca on the volcano Pichincha. One group stood alone, and reminded one of masts and stems of trees. The steep walls led us through the snow region to a narrow ridge of rock extending towards the summit, by which alone it was possible for us to advance any farther; for the snow was then so soft that one scarcely dared to tread upon its surface. The ridge consisted of very weathered crumbling rock. It was often vesicular like a basaltic-amygdaloid.

The path became more and more narrow and steep. The natives forsook us all but one at the height of 15,600 feet. All entreaties and threats were unavailing. The Indians maintained that they suffered more than we did from breathlessness. We remained alone, Bonpland,—our amiable friend the younger son of the Marquis of Selvalegre, Carlos Montufar, who, in the subsequent struggle for freedom, was shot, (at the command of General Morillo),—a Mestize from the neighbouring village of San Juan,—and myself. We attained, with great exertion and endurance, a greater height than we had dared hope to reach, as we were almost entirely wrapped in mist. The ridge (very significantly called, in Spanish, Cuchilla, as it were the knife-back) was in many places only eight to ten inches broad. On the left the precipice was concealed by snow, the surface of the latter seeming glazed with frost. The thin icy mirror-like surface had an inclination of about 30°. On the right our view sank shuddering 800 or 1000 feet into an abyss out of which projected, perpendicularly, snowless masses of rock. We held the body continually inclined towards this side, for the precipice upon the left seemed still more threatening, because there no chance presented itself of grasping the toothed rock, and because, further, the thin ice-crust offered no security against sinking in the loose snow. Only extremely light porous bits of dolerite could we roll down this crust of ice; and the inclined plane of snow was so extended that we lost sight of the stones thus rolled down before they came to rest. The absence of snow, as well upon the ridge along which we ascended, as upon the rocks on our right hand towards the east, cannot be ascribed so much to the steepness of the masses, and to the gales of wind, as to open clefts, which breathe out warm air from deeper situated beds. We soon found our further ascent more difficult, from the increase of the crumbling nature of the rock. At single and very steep *échélons* it was necessary to apply at the same time the hands and feet, as is so usual in all alpine journeys. As the rock was very keenly angular, we were painfully hurt, especially in the hands. Leopold Von Buch and I suffered very much in this manner near the crater of the Peak of Teneriffe, which abounds in obsidian. I had had besides (if it be permitted a traveller to mention such unimportant particulars) for several weeks a sore in the foot, occasioned by the accumulations of Niguas * (*Pulex penetrans*), and much increased by fine dust of pumice-stone during measurements in Llano de Tapia. The little adhesion of the rocks upon the ridge now rendered greater caution necessary, as many masses which we supposed firm lay loose and covered with sand. We proceeded one after the other, and so much the more slowly, as it was needful to try the places which seemed uncertain. Happily the attempt to reach the summit of Chimborazo was the last of our mountain journeys in South America; hence previous experience guided us, and gave us more confidence in our powers. It is a peculiar character of all excursions in the Andes, that above the snow-line white people find themselves in the most perilous situations, always without guides, indeed without any knowledge of localities.

We could see the summit no longer, even for a moment only at a time, and were hence doubly curious to know, how much higher it remained for us to ascend. We examined the barometer at a point where the breadth of the ridge permitted of two persons standing conveniently together. We were now at an elevation of 17,300 feet; thus scarcely two hundred feet higher than we had been two months before, when climbing a similar ridge on the Antisana. It is with the determining of heights in climbing mountains, as with the determining of temperature in the heat of summer. One finds with vexation the thermometer not so high, the barometer not so low, as one expected. As the air, notwithstanding the height, was quite saturated with moisture, we now found the loose rock and the sand that filled its interstices extremely wet. The air was still 2° 8′ (37° 04′ Fahr.) Shortly before, we had in a dry place been able to bury the thermometer three inches deep in the sand. It indicated + 5° 8′ (+ 42° 44′ Fahr.) The result of this observation, which was made at the height of about 2860 toises, is very remarkable, for 400 toises lower down, at the limit of perpetual snow, the mean heat of the atmosphere is, according to many observations, carefully collected by Boussingault and myself, only + 1° 6′ (34° 88′ Fahr.) The temperature of earth (sand) at + 5° 8′ (42° 44′ Fahr.) must therefore be ascribed to the subterranean heat of the dolerite mountain; I do not say to the whole mass, but to the current of air ascending from the interior.

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* The Sand-flea, the Chique of the French colonists of the West Indies, an insect that introduces itself under the human skin, and, as the ovary of the impregnated female considerably enlarges, inflammation is excited.

After an hour of cautious climbing, the ridge of rock became less steep; but alas! the mist remained as thick as ever. We now began gradually to suffer from great nausea. The tendency to vomiting was combined with some giddiness; and much more troublesome than the difficulty of breathing. A coloured man (a mestize of San Juan), not from selfish motives, but merely out of good nature, had been unwilling to forsake us. He was a poor vigorous peasant, and suffered more than we did. We had hæmorrhage from the gums and lips. The conjunctiva of the eyes likewise, was, in all, gorged with blood. These symptoms of extravasation in the eyes, and of oozing from the lips and gums, did not in the least disquiet us, as we had repeatedly experienced them before. In Europe, M. Zumstein began to experience hæmorrhage at a much lower elevation on Monte Rosa. The Spanish warriors during the conquest of the equinoctial region of America (during the Conquista), did not ascend above the snow line, thus but little above the elevation of Mont Blanc, and yet Acosta, in his *Historia Natural de las Indias*,—a kind of physical geography, which may be called a master-piece of the 16th century,—speaks circumstantially of "Nausea and Spasm of the Stomach," as painful symptoms of the *mountain-sickness*, which in these respects is analogous to *sea-sickness*. On the volcano of Pichincha I once felt, without experiencing hæmorrhage, so violent an affection of the stomach, accompanied by giddiness, that I was found senseless on the ground, just as I left my companions on a wall of rock above the defile of Verde-Cucha, in order to perform some electrical experiments on a perfectly open space. The height was inconsiderable, below 13,800 feet. But on the Antisana, at the considerable elevation of 17,220 feet, our young travelling companion, Don Carlos Montufar, bled freely from the lips. All of these phenomena vary according to age, constitution, the tenderness of the skin, the preceding exertions of the muscular powers; yet for single individuals they are a kind of measure of the atmospheric tenuity, and of the absolute elevation reached. According to my observations in the Cordilleras, these symptoms manifest themselves in white people, with a mercurial column between 14 inches,—and 15 inches 10 lines. It is known that the estimates regarding heights, which æronauts maintain that they have reached, generally deserve but little credit, and if a more certain and extremely accurate observer, M. Gay Lussac, who, on the 16th of September 1804, reached the vast height of 21,600 feet (thus between the height of Chimborazo and Illimani), experienced no hæmorrhage, this is perhaps to be ascribed to the absence of muscular exertion. According to the present condition of eudiometry, the air of those lofty regions appear to contain in proportion as much oxygen as that of lower heights; but since in that attenuated air—the barometric pressure only one-half of that to which we are generally exposed—the blood in each act of respiration takes up a smaller quantity of oxygen, it is certainly conceivable that a general feeling of weakness should take place. Why this asthenie as in fainting, should excite nausea and a tendency to vomiting, it is not our purpose to determine; as little is it here to be proved, that the oozing of blood (the hæmorrhage from the lips, gums, and eyes), which also has not been experienced by all, at such great heights,—can by no means be satisfactorily explained by the absence of a "mechanical counter-pressure" on the vascular system; our attention should rather be engaged in examining the probability of the influence of a diminished atmospheric pressure, during fatigue, on the moving of the legs in regions of very attenuated air; for, according to the memorable discovery of two spirited inquirers, Wilhelm and Edward Weber,* the hovering leg, hanging from the trunk, is held and carried merely by the pressure of the atmosphere.

The layers of mist that prevented our seeing distant objects, appeared suddenly, notwithstanding the total stillness of the air, perhaps through electrical processes, to be broken up. We recognised once more, and indeed immediately before us, the dome-shaped summit of Chimborazo. It was an earnest, momentous gaze. The hope to reach this summit animated our powers anew. The ridge of rock, only here and there covered with thin flakes of snow, became somewhat broader. We hastened onwards, with certain steps, when all at once a ravine of some 400 feet in depth, and 50 broad, set an insurmountable barrier to our undertaking. We saw distinctly beyond the abyss, our ridge of rock continued forward in the same direction; yet I doubt its leading to the summit itself. The chasm was not to be gone round. On the Antisana, M. Bonpland indeed had found it possible, after a very cold night, to proceed for a considerable length through the snow. We durst not venture the attempt, because of the looseness of the mass, and the form of the precipice rendered climbing down impossible. It was one o'clock in the afternoon. We set up with much care the barometer. It indicated 13 inches 11$\frac{9}{10}$ lines. The temperature of the air was now — 1° 6′ (+ 29° 12′ F.), but after several years' stay in the hottest regions of the Tropics, this small degree of cold benumbed us. Besides, our boots were thoroughly soaked with snow-water, for the sand that covered here and there the ridge was mixed with old snow. According to La Place's barometrical formula, we had reached a height of 3016 toises, or more precisely 18,097 Paris feet. If La Condamine's estimate of the height of Chimborazo, as noted on the stone-table of the Jesuit's College in Quito, be correct, there failed us yet of the summit of the mountain 1224 feet, or thrice the height of St Peter's church at Rome.

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* *Mechanik der Merschlichen Gehwerkzeuge.* 1836. § 64. S. 147–160. More recent experiments of the brothers Weber, at Berlin, have fully confirmed the position, that the leg is carried in the acetabulum by the pressure of the atmosphere.

La Condamine and Bouguer say explicitly, that they attained, on Chimborazo, the height of 2400 toises only; but they glory in having seen on the Corazon,—one of the most picturesque snow-mountains (Nevados) in the immediate neighbourhood of Quito,—the barometer at 15 inches 10 lines. They say this is "a lower state than any human being has hitherto ever been in a situation to observe." At the above described point on Chimborazo, the pressure of the air was about two inches less; less also than at the highest point reached in 1818, sixteen years afterwards, by Captain Gerard, on the Tarhigang, in the Himmalayan Mountains. I have been exposed in a diving-bell in England to an air-pressure of forty-five inches, for almost an hour together. The flexibility of the human organism consequently endures changes in the state of the barometer, amounting to thirty-one inches. Yet the physical constitution of the human race might be remarkably changed, if great cosmical causes were to make permanent such extremes in atmospheric tenuity and condensation.

We remained but a short time in this mournful solitude, being soon again entirely veiled in mist. The humid air was not thereby set in motion. No fixed direction was to be observed in single groups of the denser particles of vapour; I therefore cannot say whether at this elevation the west wind blows, opposing the Tropical monsoon. We saw no longer the summit of Chimborazo, none of the neighbouring snow-mountains, still less the table-lands of Quito. We were as though isolated in a ball of air. Some stone-lichens only had followed us above the line of perpetual snow. The last cryptogamic plants which I collected were Lecidea atrovirens (Lichen geographicus, *Web.*), and a Gyrophora of *Acharius*, a new species (Gyrophora rugosa), at about the height of 2820 toises. The last moss, Grimmia longirostris, grew 400 toises lower down. A butterfly (sphinx) was caught by M. Bonpland at the height of 15,000 feet; we saw a fly 1600 feet higher. The following facts afford the most striking proof that these animals are involuntarily carried up into those upper regions by the current of air which rises from the warmed plains. As Boussingault ascended the

Silla de Caracas, to repeat my measurement of the mountain, he saw from time to time, at the height of 8000 feet, at noon, as the west wind blew, whitish bodies rapidly pass through the air, which he at first took for soaring birds with white plumage, that reflected the sun's rays. These bodies rose with great rapidity out of the valley of Caracas, and surmounting the summit of Silla, took a north-east direction, and reached probably the sea. Some fell upon the southern acclivity of the Silla; they were grass-halms, that had reflected the sun's rays. Boussingault sent me some of these, which still had ears, in a letter to Paris, where my friend and fellow-labourer Kunth instantly recognised them as the Wilfa tenacissima, which grows in the valley of Caracas, and which he has described in our work, *Nova Genera et Species Plantarum Americæ Æquinoctialis.* I must remark, that we met with no condor on Chimborazo, that powerful vulture, which is so frequent on Antisana and Pichincha, and which shews great confidence from its ignorance of man. The condor loves pure air, in order the easier from on high to recognise its prey or its food, for it gives dead animals the preference.

As the weather became more and more cloudy, we hastened down upon the same ledge of rock, that had favoured our ascent. Caution, however, on account of the uncertainty of the steps, was more necessary than in climbing up. We tarried only just to collect fragments of rock. We foresaw, that in Europe "a little bit of Chimborazo" would be asked for. At that time, no mountain rock in any part of South America had been named; the rocks of all the high summits of the Andes were called granite. As we were at the height of about 17,400 feet, it began to hail violently. The hailstones were opaque, and milk-white, with concentric layers. Some appeared considerably flattened by rotation, twenty minutes before we reached the lower limit of perpetual snow, the hail was replaced by snow. The flakes were so dense, that the snow soon covered the ridge of rock many inches deep; we should have been brought into great danger, had the snow surprised us at the height of 18,000 feet. At a few minutes after two o'clock, we reached the point where our mules were standing. The natives that remained behind, had been very apprehensive for our safety.

That part of our expedition which lay above the snow-line, had lasted only 3¼ hours, during which, notwithstanding the tenuity of the air, we had not found it needful to take rest by sitting down. The diameter of the dome-shaped summit at the snow-line—*i. e.* at the height of 2460 toises—amounts to 3437 toises, and near the apex, about 150 toises below the same, the diameter is 672 toises. The last number is thus the diameter of the upper part of the dome or bell; the first expresses the breadth, of which the whole snow-mass of Chimborazo appears to the eye, as seen from Rio Nuevo; a mass which, together with the two mountain-tops lying to the north, is represented in the 16th and 25th table of my engraved work, *Vues des Cordillères.* I have carefully measured with the sextant, the single parts of the contour, as the latter, on a clear day, magnificently stands forth in opposition to the deep-blue of a tropical sky. Such observations assist in thoroughly exploring the volume of this colossus, in so far as it surmounts a plain, in which Bonguer performed his experiments on the attraction of the mountain for a pendulum. A distinguished geognost, M. Pentland, to whom we are indebted for a knowledge of the heights of Sorata and Illimani, and who, furnished with excellent instruments for astronomical and physical research, is now again going to upper Peru (Bolivia) has assured me, that my figure of Chimborazo is, as it were, repeated in the Nevado de Chuquibamba, a trachyte mountain of the Western Cordilleras, north of Arequipa, having a height of 19,680 feet (3280 toises). Next to the Himmalayan mountains, this is, owing to the frequency of high summits and the mass of the same, between the 15th and 18th degree of south latitude the greatest enlargement on the earth's surface, with which we are acquainted, in so far namely, as this enlargement proceeds, not from the primitive form of the revolving planet, but from the elevation of mountain-chains and single domes of dolerite, trachyte, and albite rock, within these mountain-chains.

On account of the snow newly fallen, we found in our decent from Chimborazo, the lower limit of perpetual snow, in accidental and temporary conjunction with the deeper sporadial spots of snow on the naked lichen-covered rocks, and on the grass plain (Pajonal); yet it was always easy to recognise the proper limit of perpetual snow (then at the height of 2470 toises) by the thickness of the bed and by its peculiar state. I have shewn, in another place (in a treatise on the causes which conditionate the curvature of *isothermal lines* incorporated into one of the *fragmens Asiatique*), that in the province of Quito, the differences in height of the snow-line on the different *Nevados*, according to the sum-total of my measurements, varies only about 38 toises,—that the mean height itself is to be reckoned 14,760 feet, or 2460 toises,—and that this limit in Bolivia, 16° to 18° south of the equator, on account of the relation of the mean annual temperature to the mean temperature of the hottest months, on account of the mass, extent, and greater height of the surrounding heat-radiating *plateaus*, on account of the dryness of the atmosphere, and the complete absence of any falling snow between March and November, lies at a height of full 26,780 toises. The lower limit of perpetual snow, which by no means coincides with the isothermal curve of 0°, rises consequently higher, as an exception, instead of falling, as one recedes from the equator. From quite analogous causes of the radiation of heat in neighbouring table-lands, the snow-line lies between 30¾° and 31° of northern latitude, on the northern Thibet side of the Himmalayan range, at the height of 2600 toises; while on the southern Indian side, it reaches the height of only 1950 toises. Through this remarkable influence of the shape of the earth's surface, a considerable part of inner Asia, beyond the Tropics, is inhabited by an agricultural population, who, though monk-governed, are advanced in civilization; where in South America, under the equator, the ground is covered with eternal ice.

We took a somewhat more northern way back to the village of Calpi than the Llanas de Sisgum, through the Paramo de Pungupala, a district rich in plants. By five o'clock in the evening we were again with the friendly clergyman of Calpi. As usual, the misty day of the expedition was succeeded by the clearest weather. On the 25th of June, at Riobamba Nuevo, Chimborazo presented itself in all its splendour,—I may say, in the calm greatness and supremacy which is the natural character of the tropical landscape. A second attempt upon a ridge interrupted by a chasm, would certainly have turned out as fruitless as the first, and I was already engaged with the trigonometrical measurement of the volcano of Tungurahua.

Boussingault, on the 16th of December 1831, with his English friend Colonel Hall,—who was soon afterwards assassinated in Quito,—made a new attempt to reach the summit of Chimborazo, first from Mocha and Chillapullu, then from Arenal, thus by a different way from that trodden by Bonpland, Don Carlos Montufar, and myself. He was obliged to give up the ascent, when his barometer indicated 13 inches 8½ lines, with an atmospheric temperature of + 7°.8 (+ 46°.04 F.). He thus saw the uncorrected column of mercury almost three lines lower, and reached a point 64 toises higher than I did, viz. 3080 toises. Let us have the words of this well-known traveller of the Andes, who was the first to carry a chemical apparatus to, and into, the craters of volcanoes. " The way," says Boussingault, " which we opened for ourselves through the snow, in the latter part of our expedition, permitted of our advancing but very slowly. On the right we were enabled to grasp hold of a rock, on the left, the abyss was fearful. We were already sensible of the effect of the attenuated air, and were obliged, every two or three steps, to sit down. As soon, however, as we were seated, we again stood up, for our sufferings lasted only while we moved. The snow we were obliged to tread was soft, and lay three or four inches deep, on a very smooth and hard covering of ice. We were obliged to hew out steps. A Negro went before, to perform this work, by which his powers were soon exhausted. As I was endeavouring to pass him, for the purpose of relieving him, I slipped, and happily was held back by Colonel Hall and my Negro. We were (adds M. Boussingault) for a moment all three in the greatest danger. Further on, the snow became more favourable, and at three quarters past three o'clock we stood upon the long-looked for ridge of rock, which was only a few feet broad, and surrounded by immeasurable depths. Here we became convinced that to advance farther was impossible. We found ourselves at the foot of a prism of rock, whose upper surface, covered with a cap of snow, forms the proper summit of Chimborazo. To have a true figure of the topography of the whole mountain, one must imagine an enormous snow-covered mass of rock, which from all sides appears as if supported by buttresses. The latter are the ridges, which, adherent, project through the eternal snow." The loss of a natural philosopher, like Boussingault, would have been indescribably dearly-bought with the little gain which undertakings of this sort can afford to science.

Although, thirty years ago, I expressed the wish that the height of Chimborazo might be again trigonometrically measured, there yet remains some uncertainty as to the absolute result. Don Jorge Juan and the French Academicians, after different combinations of the same elements, or at least after operations, the whole of which were in common, give the heights of 3380 and 3217 toises; heights which present a difference of $\frac{1}{20}$th. The result of my trigonometrical operation (3350 T.) falls between them, but approaches to within $\frac{1}{112}$th of the Spanish estimate. Bouguer's lesser result is founded, in part at least, upon the height of the city of Quito, which he estimated at 30 to 40 toises too low. He gives, according to old barometric formula, without correction for the temperature, the height of 1462, instead of 1507 and 1492 toises, the very accordant results, respectively, of Boussingault's observations and my own. The height at which I estimate the plain of Tapia, where I measured a base of 873 toises in length,* also appears to be pretty free from error. I found the same to be 1482 toises; and Boussingault, at a very different season of the year, and thus with other diminutions of temperature in the atmospheric strata, 1471 toises. Bouguer's operation was, on the other hand, very complicated, as he was obliged to estimate the height of the valley-plain, between the eastern and western Andes, by means of very small angles of height of the trachyte-pyramid of Ilinissa, measured in the under region of the coast. The only considerable mountain of the earth, of which the measurements now agree within $\frac{1}{248}$th, is Mont Blanc; for Monte Rosa was, with four different series of triangles by an excellent observer, the astronomer *Carlini*, estimated at 2319, 2343, 2357, and 2374 toises; by *Oriani*, likewise by triangulation, at 2390 toises; differences of $\frac{1}{34}$th. The oldest detailed mention of Chimborazo, I find to be that of the spirited, somewhat satyrical, Italian traveller, Girolamo Benzoni, whose work was printed in 1565. He says, that the Montagna di Chimbo, 40 *miglia* high, appeared to him strangely *come una visione.* The natives of Quito knew, long before the arrival of the French surveyors, that Chimborazo was the highest snow-mountain in all their country. They saw that it ascended highest above the line of perpetual snow. It was just this consideration that induced them to consider the now fallen in Capac Urcu as higher than Chimborazo.

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* *Humboldt*, Recueil d'observations astronomiques, d'operations trigonometriques, etc. T. I. p. lxxii.

Regarding the geognostical constitution of Chimborazo, I here add only the general remark, that if, according to the important results which Leopold von Buch has laid down in his classical memoir, "On Craters of Elevation and Volcanoes,*" Trachyte is a mass containing Felspar, and Andesite a mass with imbedded Albite; the rock of Chimborazo is by no means deserving of either name. That in Chimborazo, Augite replaces Hornblende, the same intelligent geognost observed, more than twenty years ago, when I requested him to examine, oryctognostically and with precision, the rocks brought home by me from the Andes. This fact has been mentioned in several parts of my "*Essai geognostique sur le Gisement des Rochers dans les deux Hémisphères*," which appeared in the year 1823. Besides this, my Siberian travelling companion, *Gustav Rose*, who, by his excellent work on the minerals related to felspar, and their association with augite and hornblende, has opened new ways for geognostical research, finds in all my collection of mountain-fragments from Chimborazo, neither albite nor felspar. The whole formation of this celebrated summit of the Andes, consists of labrador and augite; both fossils recognisable in distinct crystals. Chimborazo is, according to the nomenclature of *Gustav Rose*, an augite-porphyry, a species of dolerite. Obsidian and pumice stone are also wanting in it. Hornblende occurs very sparingly. Chimborazo is thus, as taught by Leopold von Buch's and Elie de Beaumont's latest decisions, analogous in its rock to Etna. With the ruins of the old city of Riobamba, three geographical miles east of Chimborazo, there is associated true diorite-porphyry, a mixture of black hornblende (without augite) and white glassy albite, a rock which reminds one of the beautiful columnar masses of Pisoje near Popayan, and of the Mexican volcano of Toluca; which also, I ascended. Some of the pieces of augite-porphyry, which I found as high up as 18,000 feet upon the ridge of rock leading towards the summit, for the most part in loose pieces, of from 12 to 14 inches in diameter, are minutely porous, and red in colour. These pieces have shining vesicular cavities. The blackest are sometimes light, like pumice-stones, and as if recently changed by fire. They have not, however, flown in streams like lava, but have probably been thrust out through fissures, on the side of the earlier raised-up dome-shaped mountain. The whole table-land of the province of Quito has always been considered by me as a great volcanic area. Tungurahua, Cotopaxi, Pichincha, with their craters, are only different openings of this area. If volcanism, in the broadest sense of the word, marks all the appearances which depend on the reaction of the interior of a planet on its oxydized surface, this part of the high land is more exposed than any other in the tropical region of South America, to the effect of this volcanism. The volcanic powers rage also, under the domes of augite-porphyry, which, like that of Chimborazo, have no crater. Three days after our expedition, we heard, in New Riobamba, at one o'clock A. M., a raging subterranean crash *(bramido)* that was accompanied by no concussion. Three hours later, there followed a violent earthquake, without any preceding noise. Similar *bramidos*, coming, as it is supposed, from Chimborazo, were perceived some days before at Calpi. Nearer to this mountain-Colossus, in the village of San Juan, they are extremely frequent. They excite the attention of the natives no more, than distant thunder out of a deeply-clouded sky does in our northern zone.

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* *Poggendorff's* Annalen, Band. 37. S. 188—190. Also Edinburgh New Philosophical Journal, for translation of this memoir.

These are the few fugitive remarks on two ascents of Chimborazo, which I have allowed myself to communicate from an unprinted journal. Where Nature is so mighty and so vast, and our endeavours are purely scientific, the exhibition of any ornament in language may well be spared.

Meteorological Observations made at Heriot Row, Edinburgh, by a Committee of the Physical and Mathematical Society of Edinburgh. 162 feet above the level of the Sea.*

Hour.	Barom. corrected.	Attached Therm.	External Therm.	Leslie's Hygrom.	Rain in inches.	Wind.		Remarks.
						Direction.	Force.	
1836. Dec. 21. 6 A. M.	29.917	55°.1	46°.4	7°.0	0.00	W.S.W.	Moderate	Generally cloudy.
7	29.913	53.2	47.0	8.7	...	do.	do.	Do. clearing in E.
8	29.922	58.0	46.8	9.0	...	do.	do.	Do. lighter in zenith.
9	29.919	57.8	47.3	8.5	...	W.	do.	Do. do.
10	29.985	54.3	47.0	7.9	...	do.	do.	Horizon cloudy.
11	29.986	56.8	47.2	9.5	...	do.	do.	Do. otherwise clear.
12	29.992	57.2	47.8	8.2	...	do.	Light	Do. do.
1 P. M.	30.012	56.7	48.1	8.6	...	do.	do.	Do. sun shining.
2	30.038	56.5	49.0	9.8	...	do.	do.	Sky dull.
3	30.104	56.5	48.4	9.6	...	do.	do.	Do. clearer to the N.
4	30.010	57.0	48.0	10.3	...	do.	do.	Clearer, clouds breaking up.
5	30.062	56.5	48.1	6.2	...	do.	do.	Clear, but cloudy horizon.
6	30.077	55.2	47.0	7.5	...	do.	Brisk	Moon shining, light clouds flitting across.
7	30.097	56.8	46.8	3.5	...	do.	Light	Do. deposition of dew (?).
8	30.090	57.2	46.2	4.5	...	do.	do.	Do. horizon cloudy.
9	30.137	58.5	46.5	5.8	...	do.	do.	Do. clearer in E.
10	30.130	59.0	46.8	7.0	...	do.	Brisk	Do. do.
11	30.103	58.2	46.4	5.0	...	do.	Scarcely any	Cloudy, deposition of dew (?).
12	30.123	57.8	46.8	4.5	...	W. by S.	do.	Do. a few drops of rain.
... 22. 1 A. M.	30.105	58.3	48.2	7.0	...	do.	do.	Do.
2	30.108	59.8	49.0	6.0	...	W.S.W.	Moderate	Do. small drizzling rain.
3	30.091	59.5	49.0	6.5	...	W.	do.	Moon shining, cloudy in E. and N.
4	30.055	57.7	48.6	9.5	...	W.S.W.	do.	Do. with halo.
5	30.073	57.0	49.2	11.5	...	W.	do.	Cloudy on horizon, stars visible.
6	30.048	59.5	48.7	10.3	...	W.S.W.	do.	Cloudy, moon obscured.
7	30.061	58.8	49.0	13.5	...	do.	do.	Clouds radiating from W., masses in N.
8	30.057	59.2	49.5	11.1	...	W.	do.	Sky covered with heavy masses of cloud.
9	30.029	58.8	49.1	11.2	...	do.	do.	Clouds breaking up in zenith.
10	30.012	56.6	49.5	15.0	...	do.	Light	Clearer, horizon cloudy.
11	30.014	57.8	49.3	18.5	...	do.	do.	Do. to N.W.
12	29.968	57.6	49.5	21.5	...	do.	do.	Rainy clouds, chiefly to N.W.
1 P. M.	29.922	58.0	49.5	22.0	...	W.N.W.	do.	Do. dark and rainy to N.W.
2	29.899	57.6	49.5	18.3	...	W. by N.	Moderate	Do. very rainy to N.W. and S.W.
3	29.852	57.2	49.0	11.1	...	W.	do.	Do. clearer.
4	29.823	57.2	49.0	12.0	...	W. by N.	do.	Generally cloudy.
5	29.784	58.5	48.8	9.0	...	do.	do.	Do.

Hour.	Barom. corrected.	Attached Therm.	External Therm.	Leslie's Hygrom.	Rain in inches.	Wind.		Remarks.
						Direction.	Force.	
1837. March 21. 6 A. M.	29.865	46°.9	32°.5	9°.0	0.00	S.S.W.	Light	Dull, and generally cloudy.
7	29.859	49.3	33.0	8.2	...	S.W.	do.	Clearing up in all directions.
8	29.839	50.3	34.8	14.8	...	W.S.W.	do.	Sky hazy all round.
9	29.809	50.7	38.0	17.8	...	W. by S.	do.	Sky clear, hazy along the horizon.
10	29.792	49.8	39.0	20.2	...	W.N.W.	do.	Generally hazy.
11	29.760	50.5	40.3	22.9	...	do.	Brisk	Do.
12	29.740	46.9	39.8	15.2	0.01	do.	Moderate	Generally cloudy, rain with snow.
1 P. M.	29.701	50.9	40.5	18.0	0.01	do.	do.	Do. rain.
2	29.675	47.7	42.4	19.5	...	do.	do.	Do. do.
3	29.648	50.5	41.6	16.0	0.01	N.W.	do.	Do. do. clearer in N. W.
4	29.645	47.1	42.4	17.5	...	N.N.E.	do.	Do. do. more cloudy.
5	29.639	47.5	41.2	14.0	...	N.	do.	Do. do.
6	29.643	50.2	41.0	15.5	0.01	do.	do.	Do. clearer in the zenith.
7	29.642	50.4	40.2	15.5	...	do.	do.	Do. rain.
8	29.642	52.4	39.7	15.0	0.01	do.	do.	Do. heavy rain.
9	29.645	53.6	38.4	11.5	...	do.	Light	Do. clearer in zenith, fair.
10	29.636	49.0	37.8	11.0	0.01	N.N.E.	do.	Do. rain.
11	29.640	53.5	37.5	11.7	...	N. by E.	do.	Do. do.
12	29.640	52.2	35.5	5.5	0.01	S.E.	do.	Do. do.
... 22. 1 A. M.	29.613	53.5	35.0	7.0	...	do.	do.	Do. fair.
2	29.650	50.0	34.5	4.0	0.02	do.	do.	Do. heavy rain.
3	29.615	53.0	35.2	8·0	...	do.	do.	Do. rain very heavy.
4	29.633	49.0	34.7	10.3	...	do.	do.	Clearer and fair.
5	29.618	49.1	34.2	12.8	...	do.	do.	Do. clearing in the E. and W.
6	29.660	50.3	33.5	12.5	...	do.	do.	Much clearer, but still cloudy, ice on the roof.
7	29.654	51.5	34.0	12.0	...	E.S.E.	do.	Cloudy, a few flakes of snow, do.
8	29.662	49.5	35.6	20.5	...	do.	do.	Cloudy and hazy.
9	29.664	51.3	37.0	17.0	...	do.	do.	Clearing up all round.
10	29.664	49.4	37.0	18.0	...	E.	do.	Thick clouds from the E., slight snow-shower.
11	29.657	50.6	38.7	4.5	...	do.	Moderate	Sunshine, but many clouds, slight hail-shower.
12	29.665	49.5	40.5	...	0.01	do.	do.	Do. fewer clouds.—N.B. Hygr. not obsd.
1 P. M.	29.644	48.9	44.0	4.0	...	do.	do.	Do. fine clear day.
2	29.664	51.9	43.1	21.5	...	E.S.E.	do.	Do. do.
3	29.684	51.5	40.8	11.0	...	S.	do.	Do. do. clouds in horizon.
4	29.691	49.9	37.6	19.3	...	N.	Light	Do. do. clouds rising ln W.
5	29.695	49.6	37.0	22.5	...	N.N.E.	do.	Clear, clouds in the horizon.
6	29.703	49.4	39.8	18.3	...	E.	do.	More cloudy.
	29.683	50.2	38.0	13.6	0.10			

Hour.		Barom. corrected.	Attached Therm.	External Therm.	Moistened Therm.	Rain in inches.	Wind.		Remarks.
							Direction.	Force.	
1837. June 21.	6 A. M.	29.628	66.3	59.5	54.0	...	S. by W.	Light	Clear and fine.
	7	29.659	66.8	62.5	55.3	...	do.	do.	Do. do.
	8	29.663	66.8	65.4	56.5	...	S.W.	do.	Do. do.
	9	29.682	66.5	64.2	56.2	...	do.	do.	Cloudy, sun obscured.
	10	29.694	66.7	66.7	57.8	...	do.	do.	Light clouds in the zenith, sun obscured.
	11	29.708	66.7	66.4	57.6	...	do.	do.	Heavy clouds from the S.W.
	12	29.721	67.8	68.9	58.4	...	do.	do.	Do. do. sunshine.
	1 P. M.	29.752	67.6	66.2	58.5	...	W.S.W.	do.	Dull, smart shower of rain.
	2	29.756	67.2	68.0	58.5	0.01	do.	Moderate	Dull and cloudy.
	3	29.774	66.8	68.8	57.5	...	W.	do.	Cloudy, rain coming on.
	4	29.788	67.3	63.0	58.0	0.01	W. by S.	do.	Generally cloudy.
	5	29.801	67.4	65.0	58.3	...	S.W.	Light	Do. but clearer.
	6	29.811	67.6	64.7	57.6	...	do.	do.	Do.
	7	29.822	67.2	62.4	57.0	...	do.	do.	Clearer from S.S.W. to N. by E., horizon dull.
	8	29.855	67.0	61.8	57.1	...	do.	do.	Clear, dark clouds to the N.
	9	29.877	67.0	61.5	55.8	...	do.	do.	Cloudy, clear in zenith.
	10	29.883	66.7	59.9	55.0	...	S.S.W.	do.	Clear, clouds from the W.
	11	29.891	66.9	57.9	54.0	...	do.	do.	Do. do.
	12	29.910	66.9	58.9	54.7	...	do.	Brisk	Do. clouds in the E.
... 22.	1 A. M.	29.928	67.5	57.3	53.6	...	do.	do.	Cloudy.
	2	29.935	67.5	58.0	53.7	...	S.W.	do.	Clear.
	3	29.946	67.2	56.1	52.9	..	do.	Light	Do.
	4	29.954	65.3	56.5	52.5	...	do.	do.	Cloudy, clear in N.E.
	5	29.929	65.0	57.6	53.5	...	W.	do.	Do. clear in the N.
	6	29.948	65.5	58.8	54.5	...	do.	do.	Do.
	7	29.912	66.8	64.0	57.5	...	None	at all	Clear and unclouded.
	8	29.984	66.2	66.2	57.5	...	W.	Light	Do.
	9	30.034	66.8	70.5	58.5	...	do.	do	Do. beautiful day.
	10	30.037	67.8	71.1	59.5	...	do.	do.	Clouds and clear sky.
	11	30.031	68.0	72.0	61.0	...	do.	do.	Sky covered with light clouds.
	12	30.038	68.0	74.0	62.0	...	do.	do.	Sun powerful, masses of light clouds.
	1 P. M.	30.045	68.0	70.0	59.5	...	S.W.	do.	Cloudy and duller.
	2	30.056	68.6	71.3	62.5	...	do.	Moderate	Clear and fine, light clouds.
	3	30.066	68.9	68.5	58.5	...	do.	do.	Do. do.
	4	30.068	68.8	69.0	59.0	...	do.	do.	Fine day, sky covered with transparent clouds.
	5	30.068	68.8	69.0	59.0	...	do.	do.	Dull, rainy towards N.W.
	6	30.063	68.7	67.9	59.6	...	W.	do.	Cloudy.
		29.884	67.2	64.5	57.0	0.02			

On the Relations of Natural Philosophy with Chemistry and the Natural Sciences. By M. Becquerel.*

Since the conclusion of the last century, both natural philosophy and chemistry have made such progress that they now afford mutual support to each other, as well as throw light upon physiology and the different branches of natural history.

For a long period of time these sciences continued isolated, because it was first of all necessary that they should be somewhat extended and developed before their mutual bearings could be discovered; in other words, it was necessary first that facts should be discovered,—should be carefully studied in their several aspects, that accuracy might be reached,—should then be classified and analyzed, so that we might become acquainted with their causes and general principles; at the same time cautiously avoiding a mistake which has too often been committed, that of assuming particular facts to be general principles.

This progress of the human mind in the study of the sciences, which was instinctively followed by Galileo and his disciples, and so happily systematized by Bacon, has become a rule of conduct from which we can scarcely depart without going wrong; but, at the same time, if the analytic method, which consists in separating that so we may more certainly arrive at principles, produces important results, we must likewise be on our guard that we do not adopt it to the exclusion of the synthetic method, which collects together the different portions, that in this way we may reach the same consummation, when we would regard the whole range of science.

At the present time it is especially necessary to have recourse to the latter method, to promote that alliance between the physical, chemical, and natural sciences, which is now almost everywhere attempted; and the more so since the facts accumulated superabound in the various departments to which they more especially belong.

Every age has its peculiar bias and its particular tastes; ours is especially interested in physical pursuits, and looks to science for practical applications and useful discoveries. Those sciences which are most elevated in rank, readily respond to this appeal.

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* Bibliotheque *Universelle* de Geneve. May 1837.

They are the pioneers of civilization; they enlighten, they guide, and sometimes even arrest, the progress of any one who would take too bold a flight towards those departments in which they preside. We now proceed to exhibit the importance of their simultaneous association, by supplying a rapid exposition of some general facts, which are calculated to elucidate the point.

All bodies around us are composed of particles which are united by certain forces, whose nature is more or less unknown, according as they are organic or inorganic. The formation of organized bodies is still enveloped in deep obscurity, whilst it is not so, at least to the same extent, with regard to that of inorganic bodies, inasmuch as a great number of facts go to prove that their particles are subjected to the action of forces which apparently have an electrical origin.

The investigations which have been made on this subject by Davy and Berzelius, have given a most extraordinary impulse to the physico-chemical sciences. The philosophers of all countries are eager, and are becoming more and more so, to co-operate in the discovery of a principle, the knowledge of which will be of the highest importance to natural philosophy, as it will enable us to resolve all questions regarding actions at a minute distance, in the way Newton has done relative to planetary attraction.

The power in virtue of which heterogeneous particles unite together in inorganic nature, is different, or at least is not precisely of the same kind as that which operates in integrant atomic attraction (attraction molèculaire). Natural philosophy, in studying the effects of this latter, makes use of cleavage, elasticity, and polarized light. Cleavage makes us acquainted with the crystalline system of mineral bodies; the phenomena of elasticity supply us with most valuable data for the discovery of the constitution of bodies in general; and a pencil of polarized light penetrating, like a probe of infinite delicacy, into a transparent body, shews us, by the modifications it undergoes, the crystalline system to which it belongs, and at the same time presents us with the means of determining all its elements.

Circular polarization enables us to study the constitution of organic compounds, since this property has been discovered in a great many solid and fluid substances, and it has been noticed that many of them, whose chemical composition is similar, do not possess it in the same degree. This difference in this respect necessarily indicates one also in the grouping of the minute particles.

We can now, with the help of circular polarization, distinguish in a great many solutions whether there is only a mixture, or chemical combination; we can trace, in the organs of vegetables, the conversion of the gum into saccharine matter, and be present, as it were, at the various elaborations of the sap.

If to these means of investigation we add the use of the microscope for the investigation of the organization of the fecula, the gluten, the pollen, and the various tissues of plants, as this has within these few years been done, we may then form some conception of the services which natural philosophy may render to organic chemistry.

The elemental powers of bodies, of whatever nature, have such direct relations with heat, that their reciprocal effects should be studied simultaneously. These effects, however, are so well known, that we need not insist upon them at present. We shall confine ourselves to the relation of some new results which demonstrate the influence which the recent discoveries in natural philosophy exercise upon physiology.

It is pretty generally known that we can now, with very great accuracy, determine the temperature of the structures and fluids in living animals, and without inducing disorder, by means of instruments which indicate instant effects, and much more accurately than is done by thermometers of the common construction. The experiments which have been made two years ago upon animal temperature, required that they should be repeated in valleys, and in the highest mountains. In proportion as one ascends, the respiration becoming quicker owing to the exertion, it is necessary to enquire if the animal temperature does not then undergo particular changes. Particular experiments have been made on this point by M. Brechet and myself at Martigny in the Valais, and at the *hospice* of the Great Saint-Bernard, where the monks, with the greatest complacency, supplied every convenience for our investigations, and several of them even assisted in our researches. We have found that there does not exist any appreciable difference betwixt the temperature of man's muscles in the valley of the Rhone and at the Great Saint-Bernard; that a sojourn of many years in the higher regions of the Alps produces no sensible modification; and that those animals which were transported from Martigny to the *hospice* did not manifest any difference in the temperature of their tissues.

Now, inquiries have also been made upon the temperature of the internal parts of animals, for the purpose of ascertaining if it were not influenced by motion, and if there did not exist an organ in which the temperature was higher than in the surrounding parts. It has been proved by experiment, that the contractions of the muscles disengage caloric; that in warm-blooded animals there exists a difference of about a degree between the arterial and the venous blood; and that the temperature in the same system, whether arterial or venous, diminishes in proportion as you recede from the heart, so that the heart is that part which is endowed with the highest temperature.

Physiological phenomena are influenced not only by heat, but also by electricity, by atmospheric pressure, and by many other physical and chemical causes. The mass of the facts which have been observed are worthy of attracting the attention of the philosopher. We shall now confine ourselves to the action of electricity, whose effects sometimes restore the phenomena of life.

When we transmit an electrical current, either in the course of the nervous ramifications or in the opposite direction, and when we maintain it either for a considerable time, or suspend it for a longer or shorter period, that we may again renew it, there are effects produced in these various circumstances which reveal to us an arrangement in the organization of the nerves, which the most minute and delicate anatomical researches, even when aided by the help of the microscope, have not been able to discover. We find that the nerve, in its longitudinal direction, if we may so express it, has a right side and a wrong, in this way at least, that a feeble electrical current is easily transmitted in the direction of its ramifications, and only with difficulty in the opposite direction. In the former alternative there is a contraction without pain, and in the latter there is pain without contraction. This fact is of great importance, for it seems to indicate that the cause, whatever it may be, which produces the contractions, emanates from the brain; and that the perception of pain, on the other hand, ascends to it from the origin of the nerves.

Physicians, then, who mean to employ electricity in the healing art, should pay attention to two especial circumstances in transmitting the current along the course of the nerve. The first refers to its direction, and the second to its continuance. Respecting the former the effects vary according to the direction; and, regarding the latter, the excitability becomes exhausted in the ratio of the time.

In the living animal there exists a power which, by degrees, repairs the injury produced on the organs of motion by the action of the current, and which begins to exercise its restorative agency so soon as the current is interrupted. This repairing principle does not cease to operate, even with life. For a time and in a certain extent, it manifests itself, even after death.

Phenomena of a different kind also supply us with information concerning the agency which electricity may play in the phenomena of life. We here allude to the powers possessed by electrical fishes, and which we have studied, along with M. Breschet, upon the shores of the Adriatic. We have determined, by the help of all the means which science puts at our disposal, that the effects produced were assuredly owing to electricity; and that the shock of the torpedo was the result of a discharge analogous to that of a Leyden jar, arranged in such a way, that the upper surface of the electric organ is the site of positive electricity, and the lower surface that of negative electricity. In suitably arranging the apparatus, we have since seen the spark which accompanies this discharge. The torpedo, then, is a true living electrical machine.

In collecting together all the observations which have hitherto been made respecting the physiological action of electricity and the powers of electrical fishes, we have succeeded in establishing a theory concerning the contractions which may satisfy the immediate requirements of science. We conceive the organic particles of the muscles and the nerves during life, and for a short time after death, as being in a state of unstable equilibrium, but which the slightest causes disturb with the greatest facility. This instability appears to be one of the attributes of life, for it ceases so soon as these same particles begin to be subjected to the influence of those forces which influence inorganic nature.

We have here been speaking of those contractions which are produced by physical stimuli of some sort; and now it may be inquired, Are the effects the same when they are called into activity by the power of the will? The answer to this question may be beyond the reach of man, but there is nothing which prevents us from endeavouring to discover some of the physical causes which contribute to the production of this great act of organic nature.

When, for example, we see, in the experiments of Galvani, of Aldini, and of Dr Andrew Ure, upon those recently executed, the different movements of the body, and especially of the face, reproduced with a frightful reality, may we not be permitted to suppose that nature, on other and common occasions elaborating the electric fluid in the brain by means which escape our observation, thereby disposes it to put certain parts in motion without interfering with the harmony of others, in a manner more regular, and, consequently, less inordinate, than the philosopher does with his apparatus?

Now, there are so many means of exciting the electrical power in the body, since the slightest derangement in their constituent parts is sufficient to disturb the state of equilibrium, that it may very readily happen that the will, by some kind of instinctive feeling, as in the torpedo, agitates some points of the brain, for the purpose of putting the electricity into motion, and which is accordingly transmitted immediately by the nerves to the muscles in which they are ramified.

To whatever side we direct our attention, we discover that the phenomena under our view depend upon some other phenomena, so very close is the alliance which subsists between the various natural agents. Phosphorescence is a good proof of this. It is this property so worthy the especial attention of the philosopher, the chemist, and the physiologist, which enables perhaps the majority of bodies to become luminous in the dark, and sometimes in open day, by the agency of heat, light, friction, percussion, compression, electrical discharges, chemical action, sometimes violent, ometimes gentle, and the vital powers when subjected to the action of the will in those animals which are endowed with the peculiar faculty.

The glimmering which appears, and which is nothing else than the electrical light which the particles of bodies permit to escape, on their losing their natural condition of equilibrium, through the agency of one of the causes we have just named, may assist in conducting us to the origin of a vast number of phenomena. Chalk, for example, and some varieties of lime, become quite luminous after exposure to the sun's rays ; hence it results that considerable masses of these substances, after being exposed for whole days to the burning rays of the sun, may spread widely, at the close of the day, a feeble phosphorescent light. Is it not to some such cause as this we must refer the phosphorescence which some travellers have observed, as in certain mountains, it has been stated, in the interior of Africa ? This phenomenon evidently allies itself to the question of the decomposition of rocks, for it would indicate either immediate decomposition, or the disintegration of the constituent parts, or a derangement in the natural state of equilibrium, one or other of which may be induced, when the atmospheric agencies are brought into operation.

In some animals, among which lampreys may be instanced, phosphorescence is the result of a chemical action, which, to a certain extent, predominates over the will, since the luminousness shews itself only after their exposure to the light of day, and yet they possess the power of insensibly diminishing it to the extent of its complete disappearance.

Some marine fishes, on the other hand, and some other organized bodies, only became phosphorescent when they have so far advanced in the decomposition which precedes putrefaction ; that is to say, in the contest which takes place between the powers of organic and those of inorganic nature.

The phosphorescence of the ocean has been observed from time immemorial. In all regions of the globe, and especially within the tropics, as soon as the day has departed, a fainter or brighter phosphorescence is seen sparkling on the face of the waters, which is produced partly by animalculæ, partly by organic matters, such as the mucosity which escapes from the surface of the watery inhabitants of the deep, and partly from physical causes which disengage the electrical fluid. The presence of organic bodies, intimately mixed with the water, cannot be doubted after what has been witnessed by M. Brechet and myself on the Brenta in September 1835.

Finally, we shall direct attention for a moment to the very gradual operations which are continually going forward at the surface and in the interior of the earth, and to the production of which all the powers of nature, such as heat, light, electricity, affinities, capillary attraction, and even organic agencies contribute. Up to the present time these have engaged but little attention, because we were not acquainted with all the agencies which co-operated in their development. In examining for example the antique bronze medals, which have been converted into protoxide of copper, without the form having undergone any other change than an augmentation of volume, all that used to be stated concerning this kind of metamorphosis was, that it was the consequence of a kind of cementation. Now-a-days we proceed farther; other analogous facts are produced, and it is at the same time shewn, that nature must employ agencies similar to those which had formerly been used. Such is the power of these agencies, that by acting slowly they lead to the formation of a great number of natural products, and in the very organs and tissues of animals and vegetables accomplish the formation of inorganic compounds, which are sometimes deposited in regular crystals, as we have an example in the eggs of the garden snail, whose external covering is internally studded with crystals of calcareous spar, as regular and transparent as those rhomboids of Iceland spar which form the ornamental specimens of our mineralogical museums.

Might we not then, it may be enquired, by the help of their slow and gradual operations, obtain an idea of the epochs in which have occurred some of the great catastrophes, which on different occasions have broken up the surface of the globe? It is known that certain rocks, and among others, some kinds of granites, are daily undergoing decomposition; this operation taking place from the surface of the earth towards the interior, it cannot be doubted is the result of atmospheric influence; and as its progress is easily traced in ground that is broken up by man's agency, and in detached blocks, it is possible to establish a relation between what has passed during a determinate number of ages, and the effects produced since the commencement of the decomposition, so that an approximate value may be procured concerning the epoch of this last phenomenon. An observation of this sort was made at Limoges in the year 1833. The cathedral of that town was built about four centuries ago of granite, which must have been procured from the quarries which were nearest to the town. In the interior of the edifice, the granite is scarcely if at all altered; whilst on the outside those places exposed to the rains and the winds, the decomposition and disintegration is, in some portions, pretty deep; in other parts again it is much less; so that the mean alteration may be valued at 8 *millimetres*. But the portion of the granite mass decomposed in the quarry is $1^m,62$. If, then, we suppose that the progress of the alteration in the mass of granite took place in the ratio of the time, the alteration must needs have commenced about eighty-two thousand years ago. It is true, that we are yet in a great degree ignorant of the real progress of the decomposition of granite in the mass; but it must assuredly have been more rapid in earlier than in later times, when the upper portions must have to a certain extent preserved those which were underneath. In this case the law must have a decreasing progression, and would yield a still higher number than that just quoted.

We have verified the supposition, that the alteration of the granite of the cathedral is of the same kind as that of the granite in masses; it results from the decomposition of the superficial portions of the felspar, which, by losing its silicate of potash, leads to the disintegration of the constituent parts of the granite. We need scarcely remark, that our chief object in supplying these results, is for the purpose of exciting geologists to pursue this kind of investigation, which may one day lead to the discovery of data which will prove valuable in elucidating the history of the earth.

The study of these very gradual operations has a bearing, the extent of which is far from being known, not more in a scientific, than in an economic point of view. The application which is about to be made of it in the extraction of some metals from their respective ores, particularly of silver, may here be noticed in illustration. The process most generally and longest employed in the management of minerals containing silver, consists in combining this metal with quicksilver, without the aid of heat, in a set of operations which has undergone few changes since the middle of the sixteenth century, the epoch of its discovery, and the theory of which is even now far from being well known. The amalgam is separated from the mineral by washing, and the silver from the mercury by heat. The amalgamation is effected in a viscous *magma* or lie, composed of the silvery mineral bruised into an impalpable powder, and likewise of mercury, common salt, sulphate of copper and of iron, of lime and of water, which are left to their mutual spontaneous action, whilst they are moreover frequently kneaded as it were under the hoofs of horses and mules. The different actions which are produced in this magma have been carefully examined, since those principles which were formerly neglected have begun to be studied. Since that time, the different causes which are sometimes opposed to the chloridation of the silver, and to the decomposition of the chloride by the mercury, whence losses sometimes result in the process, have been discovered. By removing these disturbing causes, and by reducing the problem, so to speak, to its most simple expression, silver has been extracted from a great number of ores, without the employment either of heat or of quicksilver, by employing sea-salt only, and some chemical products which are very easily procured every where, and iron, or iron and silver only in such a way that silver is extracted by the silver. All that is required, then, for the working of these valuable minerals is, that the localities whence they are procured be found in the neighbourhood of the sea, or of those great collections of rock-salt which are concealed in many parts of the world.

The first experiments were made in tubes which were almost capillary, and upon the most minute fractions of the ore. The results have been such, that after a few trials, experiments have successfully been made upon many hundreds of pounds. And it is now hoped that in a very short time, art will be able to avail itself of a process which, in addition to the pecuniary advantages it affords, will enable us to reduce those valuable ores, which now cannot be subjected either to amalgamation, or to the action of fire, more especially those which contain copper in great quantities, and whose management has always been an impossibility to metallurgists.

The importance of working minerals containing silver, without the aid of mercury, will be easily appreciated, when it is known that the registers of the mint of Potosi bear witness that between 285 and 286 millions of *marcs* of silver have been struck from the year 1570 to the year 1800, and that in the preparation of this enormous sum 286 millions of pounds of mercury have been lost, which, at the present price, represent a capital of L. 62,500,000. This immense quantity of quicksilver, used at Potosi alone, is now in the bed of the Pilcomayor, a river of Peru, into which flows all the refuse and rubbish of the silver mine. What a mine of mercury is this same, so soon as art shall have devised means for its easy and economical recovery.

In the rapid exposition we have now presented of the relations which exist between natural philosophy, chemistry, and the different branches of natural history, we imagine we have brought together a sufficient number of facts to demonstrate the necessity which now exists of drawing closer the links which unite those several sciences.

The powers which regulate organic nature have assuredly a mode of action which is peculiar to them; but, notwithstanding, they are not so independent of those which preside over the formation of inorganic compounds, that these latter do not exercise a certain influence over the others. It is by carefully collecting and analyzing all the effects which flow from the concurrence of these two very distinct kind of powers, that we shall succeed in throwing additional light upon the phenomena of life.

One of our illustrious colleagues, in demonstrating some little time ago, before the national tribunal, the necessity of extending the course of scientific study, has been the interpreter of the wants of our time; for if literary pursuits improve the moral sense, by elevating the imagination at the recollection of noble deeds, and develope the mental faculties, and supply the mind with a store of images which may afterwards contribute to enrich our thought, what influence may not the sciences also, as a whole, exercise upon civilization, those especially which have such a powerful influence upon physical pursuits, which improve the judgment, and reveal to man the secrets of creation, enlarge the range of his ideas, and fill him with admiration at all the marvels of those laws and principles he is seeking to discover.

On Metallurgical Phenomena as illustrative of Geology. By Professor HAUSMANN of Gottingen. Communicated by the Author.[*]

THERE are two means especially fitted to secure steady advances in the study of geology, and to guard against its being lost in vague hypotheses. The one consists in an accurate observation of the changes which we perceive constantly occurring on the surface of the earth: the other, in a careful appropriation of the experience with which we are supplied by skilfully conducted experiments. The conviction of the importance due to the former of these means, gave rise to the prize question proposed by the Royal Society, and which found such a satisfactory solution in Mr Von Hoff's classical treatise on the "Traditionary Changes of the actual Surface of the Earth." The desire, by the practical application of the *latter* means, to contribute something towards the illustration of geological phenomena, elicited the observations which Professor Hausmann laid before the Royal Society twenty years ago;[†] and led also to a further prosecution of the inquiry, the result of which forms the subject of the present memoir.

If our views in the science of Geology have, in modern times, undergone important changes, they have, by a skilful enlistment in their service, of the grand discoveries in Natural Philosophy and Chemistry, indisputably obtained a more solid foundation, than could formerly have been their lot; although it would be no less shortsighted than presumptuous to consider as incapable of improvement, those geological theories which have, in our day, earned the highest eulogiums, or to regard their basis as secure as that of many physical theories which can boast of mathematical support. That one particularly important change has taken place in our geological views, is attributable to the circumstance, that the empire of Neptune, which Werner had greatly extended, especially in Germany, has lost much of its power; while, on the contrary, the dominion of Pluto which, in consequence of the mighty extension of the oceanic domains, for some time appeared to totter, has not only recovered its pristine vigour, but has had its boundaries materially enlarged. The more extensive the influence is, which is in our days ascribed to fire in the formation and transformation of our planet, the greater must appear the necessity of tracing accurately its footsteps, and of investigating by what means, or in what manner its own individual changes influence extraneous matters. For effecting this purpose, an excellent means is furnished by metallurgy, inasmuch as, of all that fire can accomplish under the guidance of art, the greatest and most diversified phenomena are displayed by the processes that take place in the melting furnace.

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[*] The above memoir is the substance of a lecture which was delivered by Professor Hausmann at a meeting of the Royal Society of Sciences, held at Gottingen on the 24th December 1836, "De usu experientiarum metallurgicarum ad disquisitiones geologicas adjuvandas."

[†] *Vide* Edinburgh Philosophical Journal, for translation of Hausmann's observations referred to above.

Among the grand modern discoveries of chemistry, none can be considered of greater importance to geology, than the discovery of the metallic bases of the earths and alkalies; and even the great naturalist Sir Humphrey Davy, whose name is in an especial manner associated with this discovery, did not omit to make a happy application of it to the theory of volcanic phenomena. He remarked, that if we should suppose the metals of the earths and alkalies (of which kalium is confessedly endowed with the property of igniting immediately with water) in combination with the common metals, or metals properly so called, in large masses under the crust of the earth, and should assume that air and water come in contact with them, the operations of the subterraneous fire, and the formation of masses of a lava-like substance, would then be explicable. This assumption, to which other distinguished naturalists also have given their assent, may be transferred to the formation of that entire portion of the crust of the earth, which is composed of masses, to which we at present, with perfect justice, ascribe a fiery origin. In conformity with this view, the origin of the Plutonic and volcanic masses of the crust of the earth appears to be the result of a process of oxidation, extending around the whole nucleus of the earth, and progressing in general from the surface towards the interior.

It is assuredly not to be misunderstood that a more accurate elucidation of this theory is of the greatest importance to geology, as it forms the groundwork of all other geological views and illustrations, and relates to that process of the earth which has had the greatest influence not only on the gradual change of its surface, but also on various other relations,—an influence which it still continues to exert. Now, if that theory be assumed, we shall be compelled to assume as constituent parts of the original mass of the earth, not only the common metals and the metals of the earths and alkalies, but also the so-called metalloids, viz. sulphur, carbon, chlorine, and fluor, whose reactions on the metallic substances were assuredly not without influence in the grand process of transformation. It speaks volumes in favour of the above mentioned theory, that the oxidized substances of that part of the earth's crust which has been formed under the influence of heat, are principally those whose bases possess the greatest affinity to oxygen, particularly the earths and alkalies; whereas the larger masses of those substances which have a less intimate affinity to oxygen, to-wit, the greater proportion of the common metals, and especially those termed precious, are found, partly in the reguline condition, partly united with the metalloids, particularly with sulphur. And here it is particularly deserving of notice, that among those oxidized substances, are many to be found whose metals possess the property of extracting oxygen from water; and this holds good especially of iron and manganese, whose oxides belong to the substances, which, along with several earths and alkalies, exist in the greatest profusion in the oxydized crust of the earth. It appears further, that those substances existing in the crust of the earth in an *unoxydized* state, are met with chiefly in more or less confined spaces, which are separated from the general oxydized mass, and so disposed that we are authorised to assume that their transposition into those spaces took place at a different time from that of the formation of the rock or rocks, in which they are contained. And here it is not to be overlooked, that if we contrast the occurrence of the oxydized substances in the crust of the earth with those that are not oxydized, we must distinguish the productions of the general process of oxydation from those which owe their immediate origin to more recent and partial processes of transformation; and to this latter class, namely, belong many oxides of metals and metallic salts, found chiefly in veins, which have been and still continue to be produced, sometimes by the immediate abstraction of oxygen from air or water, sometimes by decomposition of combinations of the metals with the metalloids.

If we consider the rocks of which the larger masses of the earth's crust, formed by the influence of heat, consists, we shall find that, notwithstanding their variety, they are composed of but few different substances. In this respect the following are of the greatest importance: silica, alumina, magnesia, lime, potash, soda, iron, and manganese. As far as quantity is concerned, silica is by far the most prevalent: to it succeeds alumina, and compared with these the other component parts are on the whole but inconsiderable. Hence it is proved in what manner the mass in general was composed, out of which the above mentioned substances were formed, by the grand process of oxydation of the crust of the earth. If we compare the Plutonic with the volcanic rocks, of which the former exhibit decided proofs of earlier formation, we recognise in their composition a grand difference, which consists in this circumstance, that in a large portion of the latter class there exists a much greater quantity of oxide of iron, and a much smaller quantity of silica, than in those which form the chief mass of the former. In the Plutonic rocks, the great prevalence of silica is displayed not only in the very general appearance of quartz, but also in the great extension of the higher kinds, the bisilicate and the trisilicate. In the volcanic rocks, on the other hand, quartz seldom appears as an essential constituent part. Besides, the higher silicates are found also simple ones, sometimes in considerable quantity; and not only does iron, in various states of oxidation, enter in greater abundance into the combination of the silicates, but it displays itself also far more generally diffused, and in much larger quantities than it does in these, partly as oxide-oxidal and oxide individually—partly in union with titanic acid. If it may now be assumed that the process of oxidation of the earth's crust advances on the whole from the surface to the centre, and that, consequently, the rocks of a later creation have arisen from the oxidation of a mass which was originally further removed from the actual surface than that mass out of which those of an earlier creation have proceeded, the consequence is at once inevitable, that, in the composition of the original nucleus of the earth, iron increases from the exterior to the interior. The same fact would then hold good also of other metals which are found most abundant in veins, as we are authorized to consider the largest portion of them as of later formation than the mountain-masses in which they exist. Meanwhile, the above observation would harmonize too with the assumption, that, while the mass of the nucleus of the earth subjected to the process of oxidation had originally been of a comparatively simple nature, the more easily oxidizable ingredients were transformed in the first instance chiefly into oxidized substances, and that the process of oxidation operated more strongly upon the substances less nearly allied to oxygen, in proportion to the advancement of the process. At all events, whether we decide for the one explanation or the other, an objection against the theory of Davy is thereby obviated, viz. that, if the mass of the nucleus of the earth were chiefly composed of the bases of the earths and alkalies, it would have a much less specific gravity than any investigations into its medium density have proved.

(To be concluded in next Number.)

On the Cause of the Temperature of Hot and Thermal Springs; and on the bearings of this subject, as connected with the general question regarding the Internal Temperature of the Earth. By Professor GUSTAV BISCHOFF of Bonn. Communicated by the Author. (Continued from Vol. XX. p. 376.*

CHAP. VII.—*Do the rains and other meteoric waters cause greater variations in the temperature of the soil than in that of the air.*

The winter rains between the tropics being supposed to have the effect of cooling the springs in those regions, I made some experiments in order to ascertain, to what extent such a depression might possibly proceed.

The mean temperature at *Cumana*, according to Don Faustin

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* Owing to the loss of Professor Bischoff's manuscript somewhere between Bonn and London, a considerable time elapsed before a fresh copy could be obtained from the author, a circumstance which explains the long interval between the publication of the present and the former part of this important memoir.

Rubio,[*] is 82°.09 F.[†], the minimum being in January 80°.35, and consequently their difference 1°.73. Supposing the temperature of the air at *Cumana*, at the beginning of the rainy season, to be at the mean or even below it; if the rains be still colder, they must cool the air until the equilibrium be restored. But the rains will cool the earth's crust, so far as they penetrate into it, sooner than they will the air; and, after some time, both the air and the earth will assume the temperature of the rains. In order to be able to observe the progress of the changes of temperature in the air and in the soil, I made the following experiments.

I placed a thermometer in a glass cylinder 20 inches high, and suspended in it a second, in such a manner, that the bulb was 10 inches above the bottom of the cylinder. This thermometer was protected by a small bell-glass, which was so luted on to it, that the bulb just appeared below its edge. The thermometer at the bottom shewed 63°.0; that suspended in the air, 62°.2. I then let water of 10° fall in a fine shower into the cylinder, and quickly observed the following alterations of temperature.

THERMOMETER						
In the air, . . .	62°.2	60°.4	59°.4	55°.4	54°.3	53°.6
On the bottom, .	63°.0	54°.3	54°.0	53°.2	53°.2	53°.1

At this moment I was obliged to interrupt the experiment, as a drop of water had spirted on to the bulb of the suspended thermometer, and caused the mercury to fall to 53°.2. Had this not happened, there is no doubt that the thermometer in the air would soon have fallen as low as the one on the bottom of the cylinder. Similar changes must take place in the earth and air during the fall of rains.

Now if the cooling of the soil keeps even pace with that of the air during the winter rains, and if the only difference is, that the former is somewhat in advance of the latter, the yearly mean temperature of the soil can be depressed but imperceptively below that of the air by this cause. When the winter rains cease, the temperature of the air soon begins to rise again; but the earth remains longer at its lowered temperature on account of the evaporation. This lower temperature may perhaps last rather longer than that caused by the rains, and consequently cause a more considerable depression of the mean temperature of the soil below that of the air. The extent of the depression caused by these two circumstances might be determined, by observing the temperature of the air, the earth and the rain water, during, and a short time after, the fall of cold winter rains.

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[*] Annal de Chimie et de Phys. vol. xxii. p. 303.

[†] We again remind the reader that the degrees in this memoir are according to Fahrenheit's scale, excepting where otherwise stated.

If the mean depression of temperature during the rainy season is equal to the difference between the mean temperature of the air and that of the rain water, that depression cannot, at *Cumana*, exceed 1°.39. The difference then between the mean temperatures of the air and of the soil, during this season, can be but a small fraction of that quantity, and must, therefore, be exceedingly small. And, although single showers may lower the temperature considerably, (Von Humboldt*) for example, observed a fall of 7°.2 R. during a shower at *Cumana*, the cooling of the air and of the soil will nearly keep equal pace together.

It seems strange that rains falling from cold regions of the atmosphere should only cause a yearly depression of temperature in the air of at the utmost 1°3. R.† But when we consider that rain-drops, at the moment of their condensation, assume a higher temperature than that of the surrounding medium, by the release of latent heat, and that, in their descent, aqueous vapours are condensed upon them, the latent heat of which, becoming free, also tends to raise their temperature; we can no longer be surprised at that phenomenon. Besides, the smaller quantity of heat received from the sun, during the tropical winters, has also some share in causing that difference of 1°.3 R.

According to Chap. V, the temperature of springs is a function of the temperatures of the meteoric waters, and of the soil through which they flow. The modifications caused by the temperature of the earth on that of the meteoric waters, depends upon—

1. The capability of the strata to receive the percolating waters, which causes a longer or shorter detention of them in those strata.
2. The capacity of the channels through which the waters flow; and

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* Voyage ix. p. 20.

† This is the difference between the mean temperatures of the hottest and coldest month at *Cumana*.

3. The capacity of heat of the strata.

If the earth be very much cleft, as is the case with the chalk rocks, and if the fissures be of considerable width, the temperature of the percolating waters will be little altered; if, on the other hand, the clefts be few and inconsiderable, or the soil be composed of an earth, such as sand, which easily allows the passage of water through it, that temperature will be considerably modified.*

In order to have an idea of the probable effect in one of these cases, I made the following experiments :—

I allowed water to filter through a bell glass 14 inches high and 4.75 inches in diameter, filled with fine sand of the lignite formation. The temperature of the water which was poured upon the sand, and which filtered through it, as well as the temperature of the sand itself, was observed from time to time, before and during the experiment, with very sensitive and perfectly harmonizing thermometers; and the following results were obtained :—

First Series of Experiments with Sand moderately Damp.

Time, .	—	4	12	18	20	27	32	34	47	62 m.
Water, .	62°.6	67°.6	—	67°.3	66°.9	—	66°.6	—	65°.7	64°.2
Sand, .	54 .7	—	54 .9	57 .6	60 .6	62 .2	—	63 .5	63 .7	63 .5
Water filtered through,	—	—	—	—	57	55 .9	—	55 .7	56 .7	57

The temperature of the air during the experiment remained at 55°.4, and 6.75 lb. (of 16 oz.) of water were filtered through.

Second Series of Experiments with Sand thoroughly wetted with Water.

Time, .	—	3	11	15	19	25	28	33	36	41	45 m.
Water, .	66°.2	67°.6	67°.1	66°.9	66°.6	66°.4	66°.2	66°	65°.8	65°.5	64°.8
Sand, .	57 .6	—	57 .7	58 .3	60 .1	61 .9	63	63 .7	64 .4	64 .4	64 .4
Water filtered through,	57	57 .2	57 .6	57 .4	57 .2	57 .29	57 .6	57 .6	58 .3	59 .2	60 .1

The temperature of the air during the experiment fluctuated between 63°.50 and 62°.82, and 5625 lb. of water were filtered through the sand.

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* Thus it has been found, that, in the chalk rocks, even thermal springs may bear signs of the influence of the external temperature; as, for example, the salt springs at *Werl*, mentioned in the preceding chapter. On the other hand, in a sandy soil, where the meteoric waters are very much divided, and come in close contact with the particles of sand, the influence of the external temperature is no longer perceptible, at a very moderate depth. For this reason, springs rising through sand shew such small variations of temperature; as is the case with the springs in the neighbourhood of *Berlin* and *Potsdam*.

Third Series of Experiments with very Wet Sand.

Time, .	—	5	6	9	13	17	18½	21	22	25	28 m.
Water, .	55°.8	—	55°.4	55°.6	—	—	55°.6	—	55°.8	—	55°.8
Sand, .	87 .1	86°	85 .1	83 .5	78°.8	71°.6	69 .8	66°.2	64°.2	61°.3	59°.4
Water filtered through,	67 .3	72.3	75 .2	80 .2	80 .4	79 .9	79 .5	78 .8	78 .4	78 .1	76 .3

Time, .	30	34	38	42	44	48	50	54	57	60 m.
Water, .	55°.8	—	55 .9	—	—	56°.1	—	56°.3	56°.3	56°.3
Sand, .	57 .7	57°.2	56 .8	56°.5	56°.5	56 .3	56°.3	56 .3	56 .3	56 .5
Water filtered through,	75 .2	72 .0	70 .3	66 .6	65 .5	62 .6	62 .1	60 .4	59 .7	59 .4

The temperature of the air fluctuated, during the experiment, between 58°.6, 59°.0, and 58°.1; and 7 lb. of water were passed through.

The results of the two first experiments shew, that water, passing from a certain height through a layer of sand of a lower temperature, gives up almost all its excess of temperature above that of the sand, and passes off with nearly the same temperature as the sand originally had. But this only holds good for a certain quantity of water, and a certain length of time; for when the whole layer of sand has acquired the temperature of the water, the water must pass off with its own original temperature.

The third series exemplifies the contrary case; namely, when a cold rain falls upon a heated soil. Here also the sand decreases in temperature more considerably and more rapidly than the percolating water. The temperature of the sand continuing lower than that of the water after the filtration, was, however, caused by my only being able to observe the temperature of the sand at ten inches below its surface, on account of the shortness of the tube of the thermometer, the whole height of the column of sand being fourteen inches. It is true, so great a difference as 17°.4 R., between the temperatures of the soil and the rain, which we assumed in the third series of experiments, is seldom to be met with in nature. However, De Luc once observed a fall of temperature at *Geneva*, of 14° R. caused by rain.

The surface of the bell-glass, containing the sand, was equal to 17.7 square inches; and, considering the water filtered through, as a cylinder having a base equal to this area, we shall find the height of the column for the first series of experiments = 10,3 inches; second = 8,58 inches; third = 10,68 inches.

But the quantities of water used in the above experiments, were, in each series, greater than the yearly fall of rain at *Cumana*, which, according to Humboldt, only amounted to from seven to eight inches; and, as on sloping ground, however porous* it may be, only a small proportion of the rain-water soaks in, perhaps hardly two of those seven or eight inches actually penetrate into the earth. Admitting the probability of this supposition, scarcely one quarter of the quantity of water made to sink through the sand in one hour, in each of the above experiments, is absorbed in a whole year by the soil of *Cumana*. These considerations are already sufficient to shew, that at *Cumana*, where it very seldom rains, the temperature of the meteoric waters can have no sensible influence on the temperature of the springs.†

The quantity of meteoric water which sinks into the earth, and serves to feed the springs, may be calculated from the quantity of water they yield. I have undertaken this calculation for a district where copious springs are abundant. It comprises a part of the volcanic group in the vicinity of the *Laacher See*. I found the district drained by a brook, which receives the waters of all these springs, to measure 568,539,072 square feet: and, according to a measurement, made at a time when it had not rained for some days, the rivulet supplies 16,25 cubic Rhenish feet of water per second, or 512,460,000 cubic feet ‡ per annum. From this we learn that only 0,9 feet of the yearly fall of meteoric waters on the district drained by this brook filters into the earth, and serves for the production of springs.

The results of the above series of experiments, shew the variations of temperature, when meteoric waters only filter to the depth of fourteen inches, and then immediately rise again as springs. It is evident that the influence of the temperature of the meteoric waters on that of springs must decrease in proportion to the depth to which they filter into the earth. If the quantity of water absorbed by a certain surface, during a certain period of time, and farther, the depth to which it sinks, the capacity of heat of the soil, and the mean difference between the temperature of the meteoric waters and the strata which receive them, be known, the variations of temperature caused in the earth's crust by the meteoric waters, during a certain period, may be calculated from those data. For these variations of temperature, which take place after a certain length of time, are the same which would ensue, if the whole quantity of water were to sink to the given depth into the earth in a moment. Since the scale of the yearly variations in the temperature of springs evidently depends on the depth from which they rise, this depth may be learned from a knowledge of that scale. Supposing that springs issue with the temperature of the lowest point to which the meteoric waters have sunk, the depth at which the same scale of variations of temperature prevails, as that of a certain spring, may be found by observing the temperature of the earth, at different depths, in the vicinity of that spring. Thus, for example, a thermometer in the *Göpel* shaft of the *Trappe* coal-mine, near Welter, in the *Grafschaft Mark*, gave only 0°.37′ as the greatest difference of temperature, at a depth of 26.5 feet,* from which the origin of a neighbouring spring, having the same scale of variation of temperature, may be easily deduced. And such a deduction will, in most cases, be very near the truth; for meteoric waters, filtering in fine drops through the earth, and through the narrow fissures and clefts in the rocks, assume the surrounding temperature the more nearly the deeper they sink. But if, on reaching the lowest point of their course, they unite themselves with another more or less abundant spring, and are brought to the surface by an impervious stratum, or by hydrostatic pressure, the change hereby operated on their temperature will be the less considerable the more copious they are, and the more quickly they flow. We will suppose, for the sake of example, the mean depth from which a certain number of springs, belonging to a certain district, rise to be thirty feet. Farther, let the specific gravity of the soil be taken at 1.5, and its capacity for heat 0.22, the specific gravity and the capacity for heat of water being taken as unity.* Supposing the height of water providing a certain surface for the supply of its springs to be one foot (about as much as we found in the district of the *Brohlbach* above mentioned), we shall have the proportion between the masses of the water and earth, which come in contact with each other, as 1 : 45. Lastly, let the meteoric waters during a whole year be 9° colder than the soil, and let the mean temperature of the latter be 46°.4. Under these circumstances, if the whole quantity of water were to fall, in one moment, upon the earth, the temperature which the earth would assume would be =

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* Voyages III. 371. xi. 19.

† In the *Corderillas*, one may sometimes wander for hundreds of miles without meeting with a spring; and it is impossible that such rare springs should have a temperature lower than that of the air.

‡ This value should have been taken somewhat higher, because the re-evaporation of the water during its course was not taken into account in our calculation.

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* Poggendorff's Annal. xxii. p. 520, table.

$$\frac{1 \times 1 \times 3 \times (45 \times 0.22 \times 8)}{1 \times 1 \times (45 \times 0.22)} \text{ or } \frac{1 \times 1 \times 37.4 + (45 \times 0.22 \times 46.4)}{1 \times 1 + (45 \times 0.22)}$$

= 45°.57 Fahr. This would consequently also be the temperature which springs, coming from a depth of thirty feet, would have.

Thus, even in case the temperature of the meteoric waters throughout the year, were 9° colder than that of the soil, and that they should only sink to a depth of thirty feet, the temperature of the soil, and consequently also of the springs rising out of it, would still only be lowered 0.83. But these conditions will with difficulty be satisfied either between the tropics, or at any other point on the surface of the globe. And supposing they were satisfied, the difference thereby produced between the mean temperature of the air and that of the soil, would fall far short of 0.83, because the cooling of the air by the meteoric waters would follow but shortly after that of the earth's crust.

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* These I found to be the specific gravity and capacity for heat of the sand used in the above experiments.

† Let M and m represent the two masses, S and s their capacities for heat; T and t their temperatures: the temperature T of the two bodies, after their intermixture, will be

$$T = \frac{M S T + m s t}{M S + m s}$$

The meteoric waters must also have as little power sensibly to raise, as they have to depress, the temperature of springs. It is, therefore, equally inadmissible, that the temperature of springs, in low latitudes, should be sensibly depressed by the coldness of the winter rains, as that, in higher latitudes, it should be raised by the warmth of the rains of summer.

In the *dislocated* rocks of limestone and quader-sandstone, the relations may prove somewhat different. The fissures in these rocks being for the most part of great depth, the meteoric waters flow through their spacious channels in torrents; the influence of the waters on the temperature of the outer crust of the earth, may, therefore, be much more considerable in these rocks. But the changes effected on the temperature of the earth will still take place, but a short time before those caused in the air.

In order to form an idea of the extent of the influence of these circumstances, I chose the quader-sandstone and chalk-rocks, on the western declivity of the *Teutoburger Wald*, which are fissured to an extraordinary extent, for the theatre of my observations. Many considerable brooks rise out of these rocks, but are for the most part lost in the clefts, and reappear at the foot of the chain. I measured the waters of the *Lippe*, the *Raute*, the *Pader*, the *Alme*, and the *Heder*, and traced the boundaries of the districts drained by them, and found that nearly two feet of the meteoric waters falling in those districts are expended in the supply of those streams.* Here, then, almost twice as much of the meteoric waters sink into the crust of the earth, as in the district of the *Brohlbach*, which is so unusually rich in mineral springs.†

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* The *Pader* I measured at the end of May 1834, when it had not rained for a long time. The *Lippe* and *Alme* were measured by Dr Pieper of *Paderborn*, at my request, in the middle of July, which was remarkable for the unusual dryness of the weather, as indeed was the whole summer of 1834. It must, however, be observed, that the *Lippe* loses little of its water in summer; but that the *Alme*, on the contrary, was very much reduced at the time of the measurement. The copiousness of these rivulets was, therefore, certainly not determined too high.

† In few parts of *Germany*, where the annual fall of rain is known, does there fall a sufficient quantity to supply the springs, which rise on the western declivity of the *Tutoburger Wald.* At *Coblence*, *Manheim*, *Stuttgart*, *Tübingen*, &c., the yearly fall of rain only amounts to from 20 to 23 inches; at *Carlsruhe*, *Giengen*, *Ulm*, *Genkingen*, 24 to 35 inches. See Kämtz Lehrbuch der Meteorologie, vol. i. p. 458 and following.

But the deeper the clefts, the nearer will the temperature of the water coincide with that of the rock. And lastly, if the waters reach the regions where the increase of temperature towards the interior of the earth becomes sensible, the springs resulting from them must be thermal. This, as already mentioned, is the case with the most springs rising in the *Teutoburger Wald.* The influence of the external temperature, then, diminishes in proportion as the thermal springs exceed the mean temperature of the air. It is however easy to conceive, that water, sinking through the wide channels in the dislocated chalk rocks, into the regions where the increase of temperature towards the centre of earth begins, should reappear, in a lower situation, as a thermal spring; but should, nevertheless, shew the variations of the external temperature, as seems to be the case with the salt-springs of *Werl,* already alluded to more than once.

CHAP. VIII.—*To what depth in the crust of the earth does the influence of the external temperature continue to be felt?*

The depth to which the external temperature continues to exert its influence upon the crust of our globe cannot be the same at all points. It depends, in the first place, upon the extent of variation of temperature of the air, for the less such variation is, the less considerable will be that depth, and vice versa. Therefore the depth will vary in the same place, the extent of annual variation of temperature of the air being in the various years also various. It depends, further, on the conductibility of heat of the earthy and rocky strata composing the crust of the earth. The former of these circumstances varies according to the geographical latitude of the place and its elevation above the sea, for the nearer to the equator, and the more elevated above the sea, the less, in general, are the variations of temperature. The latter is naturally dependent solely upon local geognostical circumstances.

Between the tropics, from 11° N. Lat. to 5° S. Lat. the depth to which the external temperature continues to be felt, is scarcely so much as 1 foot; for, according to Boussingault's * observations, thermometers, placed in holes of 8 to 12 inches deep, protected from the rays of the sun by a roofing, shewed variations of at most but a few tenths of a degree. De Saussure* found in a medium latitude that at the depth of 29.5 feet there was still a variation of 2°.25 Fahr. It must, however, be remarked, that this observation was made in a well, where the influence of the temperature of the atmosphere was not precluded. Arago found at *Paris* that a thermometer did not remain constant at a depth of 25 feet below the surface. Indeed, at a depth of 86 feet, a yearly variation of $\frac{9}{128}$, or 0.0703 of Fahr. was observed. However, the observations made at *Paris* on the 20th July 1825, by sinking thermometers to different depths into the earth, shew that the direct influence of the external temperature, that is to say, independently of the temperature of the air, does not extend much below 25 feet. It was found, namely, that at a depth of

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* Annal. de Chim. et de Phys. vol. liii. p. 225, and following. In the same manner Sir Thomas Brisbane determined the temperature of the soil at *Paramatta* (New South Wales). See Edinb. Phil. Journal, x. 279.

1½ feet the temperature was	82°.06
3	72°.19
6	62°.5
10	57°.9
20	52°.76
25	52°.49
86 (in a cellar) .	53°.14

whilst a thermometer in the shade shewed 91.4, and another immersed in sand shewed, in the sun, 127.4.† D'Aubuisson estimates the depth in question at between 46 and 61 feet, and Kupffer‡ at 77 feet below the surface.

The determination of this depth is extremely difficult, as in wells, shafts, and caverns, it is scarcely possible thoroughly to prevent the influence of the external temperature; whilst the sinking of thermometers to considerable depths into the solid rock, or into the earth, and the observing of their temperature, is hardly practicable.

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* Voyages III. in the Bibliot. Britann. viii. 341.

† Annal. de Chim. et de Phys. vol. xxx. p. 398.

‡ In a late memoir in Poggendorff's Annal. vol. xxxii. p. 270, Kupffer expresses his opinion that all points of the interior of the earth, of which the greatest variation of temperature is 0.36, are situated at the same depth, whatever be the variations of temperature at the surface. He calculates this depth for points near the equator at 25,2 feet, and for the polar regions at 35,5 feet. This is contradictory to theory, as well as to the actual observations made by Boussingault between the tropics.

It has been found, from thermometrical observations, made in various *Prussian* mining establishments, situated between 50° and 51°.5 N. Lat.,* that, notwithstanding the numerous precautions employed to isolate the thermometers placed at different depths from the atmosphere, they still, in some cases, shewed great irregularities of temperature, as will be seen from the following table;

Depth below the surface at which the observation was made.	Greatest differences of temperature observed.	Depth below the surface at which the observation was made.	Greatest differences of temperature observed.
27 Paris ft. .	0.83 F.	55 Paris ft. .	6°.19 F.
32 — .	1°.8	33 — .	6°.75
32 — .	2°.25	45 — .	10°.13
63 — .	2°.25	29 — .	12°.60
28 — .	2°.81	155 — .	0.0
26 — .	3°.37	159 — .	0.0
32 — .	5°.29		

From this we see, that in the temperate zones the influence of the external temperature may be no longer perceptible at the moderate depth of 27 feet; but that, under less favourable circumstances, it may be more or less considerably felt at depths of 55 and 63 feet, or even more; and, lastly, that at 155 and 159 feet it entirely disappears.

In the years 1830–1832, observations were made with great care in the mines of the *Saxon Erzgebirge*, on the temperature of the rock at various depths.† In order, if possible, to avoid the effects of a change of air, thermometers were used of such a length, that the bulbs might be sunk 40 inches deep into holes bored in the rock, whilst the scale might be observed from without. The spaces between the thermometers and the sides of the holes were filled with dry sand. The scales were divided to tenths, by which the observations might be taken in hundredths of a degree centesimal. The utmost care was bestowed on the graduating and harmonizing of the thermometers.

In choosing situations for the thermometers, points subject to considerable draughts of air, and such as were occupied by workmen, were as much as possible avoided. They were always placed in the solid rock, and in spots above which there were no halvens or other heaps of rubbish. The holes were bored in such a manner, that the bulbs of the thermometer should lie at least 40 inches below the surface of the rock, and only such were used as were free from wet.

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* Poggendorff's Annal. vol. xxii. p. 520.

† Beobachtungen ueber die Temperatur des Gesteins in verschiedenen Tiefen in den Gruben des *Sachsischen* Erzgebirges in den Jahren 1830 bi 1832, zusammengestellt von F Reich. Freyberg. 1834.

During his observations on the hourly variations of the magnet, Reich had an opportunity of observing the rapidity with which the air operates upon the rock even at a distance of 40 inches. The air in the mine shewed 48°.6 with but slight variations, and a thermometer sunk into the rock 48°.64; but when, after 44 hours' observation of the magnet, the temperature of the air had been raised by the presence of the observers, and their two candles, to 49°.7, the thermometer in the rock, which was subject to no change of air whatsoever, was found to have risen to 48°.71, 48°.73. This discovery destroyed all hope of obtaining the temperature of the rock, quite free from the influence of the air, by sinking the thermometers even 40 inches deep into the rock. In order to ascertain whether the temperature of the rock was raised or depressed by the air, at different points near the scale of the thermometer sunk into the rock, were placed others exactly corresponding with it. Thus, if the mean temperature should be higher than that of the rock, it might be concluded that the air tended to raise the temperature of the rock; and if lower, that it caused a depression. They were, however, satisfied with knowing, whether an increase or decrease took place, without determining to what extent it was carried. The observations frequently led to no results. The thermometers near the surface were generally observed three times a-week; those situated further down, twice. Reich estimates the errors of observation in some cases at 0.18.

Another observation, which proves how deep in the earth a change of temperature in the neighbourhood, although small, if it be but of long duration, may be felt, is worthy of notice, as it shews how many unknown, as well as known circumstances, may have influenced former thermometrical observations in mines, and how little the want of concord in the results obtained from them is to be wondered at. In the mine of *Beschert Glück*, in the *Freyberg* district, a thermometer buried in the *disintegrated* gneiss, forming the floor of a cellar under a smithy, showed, in 1830, a mean temperature of 49°.79, which was much too high, compared with other observations made at the surface. This was caused by the strong fire in the smithy, the greatest proximity of which to the thermometer was 12 feet.*

I have reduced the centessimal degrees of the observations, made near the surface, at various places in the *Saxon Erzebirge*, into degrees of Reamur, and arranged them in the following table. The numbers represent the monthly mean temperature. Reich also gives the maximum and minimum of each month; but these I have omitted for shortness' sake, and have confined myself to noting, in the four last lines, the greatest yearly differences of temperature, which is of the most importance for our object. The places of observation are arranged in the order of their depth below the surface; but their elevation above the sea is at the same time marked.

Depth below the Surface	5′.5	17′.2	22′.2	23′.4	23′.1	30′.8	33′.9	34′.8	47′.7	52′.9	94′.8	121′.3
Elevat. above the Sea	1009′	2493′	945′	1440′	1381′	1718′	1754′	2322′	1395′	1431′	1447′	1322′
1830.												
January .	4°.43	...	5°.03	5°.66	...	...	...	...	...	...	...	7°.25
February .	3.57	...	4.38	5.19	...	...	...	...	...	...	...	7.25
March . .	3.82	...	4.01	4.67	...	...	...	...	...	...	...	7.24
April . .	4.88	...	4.44	5.04	...	...	...	...	...	...	...	7.26
May . .	6.00	...	5.54	5.72	...	...	...	...	...	...	...	7.26
June . .	7.29	...	6.86	6.58	...	...	...	...	...	...	...	...
July . .	8.32	...	8.08	7.36	...	3°.91	...	...	..	...	6°.81	...
August . .	9.26	...	9.09	8.28	...	4.22	...	...	...	...	6.83	...
September .	9.22		9.09	8.23		4.67		...			6.86	...
October .	8.51	5°.47	8.52	7.89	7°.26	5.29	5°.26	...	7°.26	6°.41	6.86	7.25
November .	7.79	5.47	7.73	7.27	6.98	5.46	5.22	...	7.09	6.52	6.86	7.25
December .	6.88	5.39	6.57	6.60	6.58	5.20	5.24	...	6.93	6.39	6.88	7.24
1831.												
January .	5.77	5.10	5.42	5.85	6 21	4.87	5.28	4°.25	6.81	6.34	6.86	7.26
February .	5.02	4.84	4.68	5.39	5.98	4.14	5.20	4.04	6.66	6.61	6.87	7.26
March . .	5.02	4.56	4.37	5.23	5.94	3.46	5.11	3.99	6.71	6.71	6.86	7.28
April . .	5.27	4.26	4.86	5.41	5.99	3.25	4.97	4.22	6.82	6.70	6.83	7.29
May . .	5.95	4.06	5.89	6.09	6.13	3.40	4.85	4.78	6.99	6.69	6.85	7.29
June . .	6.71	4.21	6.93	6.72	6.43	3.54	4.86	5.34	7.14	6.66	6.85	7.29
July . .	7.55	4.67	8.31	7.50	6.98	3.80	5.00	5.94	7.26	6.62	6.86	7.29
August . .	8.18	5.32	9.14	8.08	7.66	3.99	5.21	6.46	7.42	6.58	6.87	7.29
September .	7.82	5.63	9.24	8.13	7.61	4.76	5.31	6.25	7.43	6.56	6.88	7.29
October .	7.46	5.90	9.00	7.93	7.66	5.34	5.42	6.26	7.38	6.67	6.89	7.29
November .	6.22	5.94	8.25	7.48	7.30	5.65	5.54	5.18	7.38	6.74	6.90	7.30
December .	5.02	5.75	6.98	6.80	6.87	5.40	5.50	4.49	7.21	6.81	6.89	7.31

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* Perhaps also the water used for quenching and hardening may have found its way to the spot where the thermometer was placed.

Depth below the Surface	5′.5	17′.2	22′.2	23′.4	23′.1	30′.8	33′.9	34′.8	47′.7	52′.9	94′.8	121′.3
Elevat. above the Sea	1009′	2493′	945′	1440′	1381′	1718′	1754′	2322′	1395′	1431′	1447′	1322′
1832.												
January .	...	5°.47	5°.86	6°.06	6°.43	4°.86	5°.60	4°.31	7°.13	6°.79	6°.85	7°.32
February .	...	5.08	5.16	5.56	6.23	4.59	5.50	3.95	7.10	6.79	6.85	7.33
March . .	...	4.78	4.62	5.22	6.11	4.40	5.42	3.66	7.10	6.80	6.78	7.34
April . .	...	4.48	4.78	5.10	6.13	4.14	5.32	3.87	7.12	6.84	6.84	7.36
May . .	...	4.36	5.51	5.65	6.18	3.98	5.23	4.33	7.11	6.87	6.84	7.36
June . .	...	4.45	6.90	6.38	6.50	3.90	5.18	5.14	7.20	6.87	6.83	7.37
July . .	...	4.77	7.86	7.00	6.93	4.11	5.19	5.79	7.46	6.86	6.80	7.39
August .	...	5.27	8.58	7.46	7.56	5.16	5.28	6.15	7.57	6.82	6.81	7.41
September .	...	5.58	8.83	7.96	7.67	6.07	5.44	6.10	7.58	6.75	6.84	7.42
October .	...	5.73	8.16	7.83	...	5.94	5.55	5.78	7.58	6.74	6 86	7.41
November .	...	5.68	7.22	7.14	...	6.10	5.62	4.59	7.58	6.74	6.86	7.41
December .	...	5.50	6.25	6.34	...	5.82	5.59	4.31	7.47	6.73	6.86	7.42
Mean 1830	6.66	4.92	6.61	6.54	...	4.28	5.13	...	7.05	6.57	6.86	7.27
Mean 1831	6.34	5.02	6.92	6.72	6.65	4.30	5.18	5.10	7.10	6.64	6.87	7.29
Mean 1832	...	5.10	6.65	6.47	6.80	4.92	5.41	4.83	7.34	6.80	6.83	7.38
Mean in gen.	6.50	5.05	6.73	6.58	6.72	4.54	5.28	4.97	7.22	6.70	6.85	7.33
Differ. 1830	6.10	1.78	5.30	3.90	...	2.37	0.58	...	...	0.55	0.10	0.05
...... 1831	3.57	1.94	5.01	3.20	1.87	2.72	0.78	2.74	0.88	0.63	0.12	0.06
...... 1832	1.	1.41	4.43	3.06	1.82	2.34	0.48	2.72	0.55	0.14	0.10	0.14
Differ. in gen.	6.10	1.94	5.01	3.90	1.87	3.07	0.82	3.02	0.98	0.71	0.14	0.20

If we take into consideration the many known and unknown circumstances which may influence the temperature of the rock, we cannot be astonished that, in the foregoing table, the differences between maximum and minimum do not decrease in direct proportion to the depths, so that, notwithstanding the care with which the observations in the *Saxon Erzgebirge* were conducted, they still leave us in uncertainty respecting the depth to which the influence of the external temperature penetrates. Thus much, however, may be deduced from them, that, in certain circumstances, there is a yearly difference of temperature of scarcely 2° (4°.5 Fahr.) at a depth of 17 feet; whilst under other circumstances, at twice that depth, a variation of 3° (6°.7 Fahr.) may be produced. It is therefore hardly to be expected, that the depth to which the external temperature penetrates should be discovered, for any places in high latitudes, by observations in mines, wells, or caverns.

Finally, let it be observed, that the maximum in no case took place before August, and most commonly in September, October, and November; and the minimum never before February, generally in March, and in two cases as late as June. Herrenschneider at *Strasbourg* also found, from thermometrical observations at a depth of 15 feet, that, in the years 1821 and 1822, the maximum took place in September, and the minimum in February; but in 1823, the maximum happened already in August, and the minimum in January. Muncke observed the maximum in a thermometer sunk five feet deep into the earth during the years 1821–1826, twice in September, twice in August, and once in July.*

Direct observations on the temperature of the soil have been made by sinking thermometers of different lengths into the ground, by Ferguson at *Abbotshall* in *Fife*, *Scotland*,† by Rudberg in *Stockholm*,‡ and by Quetelet at *Brussels*, § the results of which are given in the following Table:

	STOCKHOLM, 59°20 N. Lat. In 1833 and 1834.			ABBOTSHALL, 55°.10 N. Lat., 50 feet above level of the Sea, In 1816 and 1817.				BRUSSELS, 50°.51 N. Lat., in a shaded situation, In 1834.			
Depth below the Surface of Earth	1 F.	2 F.	3 F.	1 F.	2 F.	3 F.	4 F.	0.5 F.	1.7 F.	2.3 F.	3 F.
January .	29°.28	31°.29	32°.74	34°.71	37°.51	40°.64	44°.01	45°.3	45°.7	43°.33	47°.42
February .	31.3	31.96	32.4	35.3	37.93	40.2	42.3	39.09	40.37	42.26	44.09
March . .	32.59	33.1	33.39	37.22	38.53	40.71	42.34	43.04	43.47	44.49	45.57
April . .	37.99	37.44	36.9	42.3	40.39	42.01	43.19	43.73	44.32	44.40	45.29
May . .	47.94	46.76	45.14	45.31	46.49	43.99	44.09	55.76	55.2	53.54	52.64
June . .	56.54	54.5	52.31	51.3	49.73	47.44	46.79	60.14	59.07	58.2	57.59
July . . .	60.5	59.0	56.9	54.61	53.63	53.32	48.6	63.64	62.79	62.92	62.1
August . .	55.6	55.44	55.2	51.71	53.14	51.3	49.73	62.86	63.39	63.36	63.39
September .	53.89	53.52	53.39	52.22	52.1	52.1	50.31	58.79	59.89	60.6	61.24
October .	48.09	48.23	49.16	46.39	49.32	49.6	49.73	52.0	53.21	55.42	56.97
November .	38.92	40.3	42.19	40.81	44.21	44.54	46.6	44.14	46.59	48.8	51.19
December .	33.41	35.14	36.94	36.79	40.41	43.92	46.22	41.36	42.62	44.63	46.79
Mean . .	43.79	43.8	43.9	44.09	45.22	47.79	46.2	51.01	51.5	52.19	52 92
Difference between Max. and Min.	31.27	27.76	24.55	19.80	16.22	13.09	7.99	25.52	23.96	22.05	20.25

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* Gehler's Neues Wörterbuch, vol. iii. p. 988.

† Ure's Chemical Dictionary. From the anomalous result of the July observation on the thermometer situated at a depth of three feet, of which no trace is to be found in any of the rest, it seems that either an error in the observation or a misprint must have crept in. For which reason Kämtz (Lehrbuch der Meteorologie, vol. ii. p. 183) adopted as the actual observation the number 8°.65, which he obtained from a formula derived by him from observation, and thus obtained for the greatest yearly difference of temperature 11°.99.

‡ Poggendorff's Annal. vol, xxxiii. p. 251.

§ Ibid. vol. xxxv. p. 139.

Thus we find that the maxima and minima take place at an earlier period than that given by the thermometrical observations made at greater depths in mines, which is in perfect accordance with the theory. We see further, that the differences between maximum and minimum decrease with the depth, and, indeed, much more rapidly than in the *Saxon* mines. This circumstance again proves that the temperature of the air has a considerable influence on observations in mines, and that a constant temperature would be found at a much lesser depth, if the thermometers could be constructed of much greater length than four feet, and the temperature of the earth be thus observed at a greater depth.

It is very remarkable that the observations made at *Abbotshall* and *Brussels* should give such a considerable increase of mean temperature at depths differing but by a few feet. As the observations at *Stockholm* shew but a scarcely perceptible increase of temperature, and as they were made with the greatest care, it is highly probable that at the other places the observations were influenced by accidental circumstances.

CHAP. IX.—*Can it be maintained with certainty that the glaciers are melted from underneath by the internal heat of the earth, and what thermometrical phenomena accompany glaciers in general?*

The phenomena under consideration has already been pointed out by De Luc,* De Saussure,† and others, who have endeavoured to account for it by the continual radiation of the internal heat of the earth. Escher, ‡ who observed the glaciers with great caution, considers this melting of the glaciers from underneath as an undeniable fact. He says, " They would soon reach the highest limits of the mountains between which they are contained, were they not melted away from underneath by the internal heat of the earth, so as to cause the underminings and fallings in, by which an equilibrium is constantly maintained in the mass of the glaciers, between the accumulations at the surface and the loss sustained from underneath. It is by this means alone that springs are enabled to take their rise under the glaciers, and to flow uninterruptedly throughout the winter. Only in such places where these high glacier-bearing longitudinal valleys of the Alps are intersected by cross valleys, or where high glacier-bearing mountains stretch down into deep valleys, will the towering glacier masses be forced down the steep declivities, and urged forwards by their own weight into the valleys situated below the limits of perennial snow, where they will continue uninterruptedly to melt away from underneath, whilst their upper surface will only be melted during the warmer seasons."

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* Rech. sur l'Atmosph. ii. 327.

† Voyages dans les Alpes, § 533.

‡ Gilbert's Annalen, lxix. p. 113.

The melting of the glaciers from underneath can only take place where the mean temperature of the soil is above 32°. For at such elevations, where it is equal to or below 32°, and where the glacier prevents the access of the warm summer air to the soil, it will no longer be possible for the ice to be melted on its under surface. Let us suppose an alpine valley, the mean temperature of which is 32°, to be covered with a glacier at a time when the temperature at the surface is also 32°, and that the masses of snow have the same temperature, the snow will then neither receive heat from, nor give heat to, the soil beneath. If the temperature of the soil be above 32°, this excess of temperature will be expended in melting a part of the snow, until the equilibrium be restored; in the contrary case, the soil will receive heat from the snow. The law of the decrease of temperature from the centre of the earth to its surface is, therefore, not permanently deranged by a covering of snow, where the mean temperature of the soil is 32°, and it is consequently impossible to conceive, that a greater quantity of heat should at any time be emitted from the interior, by which the snow should be melted. The covering of snow will produce no other effect, except that the variations of the temperature of the soil, occasioned by the variations of the seasons, will be confined within very narrow limits, or if the covering of snow be very thick, that they will entirely disappear. However, as the temperature of the surface is only the consequence of the cooling of the earth, caused by the radiation of heat from its surface, which cooling is not compensated in all climates by the radiation from the sun, it is possible that, beneath a glacier where no radiation can take place, the original temperature of the soil may be somewhat raised. At such elevations, therefore, where the mean temperature of the soil is 32°, glaciers can only be melted on their upper surface, and during the warmer seasons, but in the winter no water can possibly flow from them.

If the mean temperature of the soil in the Alps, at an elevation of 6165 feet, is 32° F. (see Chap. XVIII), glaciers which lie at that height can no longer be melted away from underneath.* It would be very interesting, for the further investigation of this question, to ascertain the height in various parts of the Alps, at which the mean temperature of the soil is zero.

The lower extremity of the great *Lammern* glacier, on the *Gemmi*, in *Switzerland*, is situated about 7000 feet above the surface of the sea.† The mean temperature of the soil at that place must, therefore, be about 34°.47. On the 5th September 1835, at a few hundred feet from the glacier, and eight feet from the bed of the brook, which at that time was dry, I found the temperature of the soil at one foot from the surface to be 41°.9; whilst at the highest point of the *Pass of the Gemmi*, which has about the same elevation above the sea, it was only 38°.5. The former of these must, therefore, have been 44°.57, and the latter 41°.27 above the mean, a result which nearly accords with the observations made at the depth of one foot, communicated in Chap. VIII. But I also observed the temperature of fifty-one fresh-water springs on the *Spital Matte*, 5887 feet above the sea on the 3d September, and found the coldest of them to be 37°.8, and as, according to my observations on the temperature of the springs of the mountains in the vicinity of my residence (see Chap. XVIII), this temperature must have been about 33°.01 above the mean, the mean temperature of that spring must have been about 36°.81.

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* Hugi (see Berghaus Annal. iii. 290), has given the height above the sea of the lower extremities of twelve glaciers in *Switzerland*. Among these the *Oberaar* glacier (7980 feet above the level of the sea) is the only one which would not melt away from underneath; all the rest are so situated as to reach below the limit of 6165 feet, and consequently into regions where the mean temperature is above 32°.0.

† I found by my barometrical measurement, made between 200 and 300 feet above its lower extremity, that its height was 7244 feet above the level of the sea.

However, as these springs rise out of the limestone rocks, which are there so enormously fissured, and therefore probably rise from a great depth, it is to be supposed that they are thermal, and that the coldest of them bears a higher temperature than the mean temperature of the place where it rises. If this is the case, it is very possible that the mean temperature of the soil on the *Spital Matte* does not exceed 32°.92, as was supposed in Chap. XVIII.

Now, if the mean temperature of the soil near the *Lammern* glacier is below 32°, it must be one of those glaciers which cannot melt from underneath.

I was enabled to assure myself of the correctness of this opinion in another manner. The brook which flows from this great and broad glacier was on the 5th September so small, that one might easily walk through it. Besides, even that small quantity of water seems for the most part to have proceeded from a waterfall, which precipitates itself into the glacier on the north side, and reappears at the lower extremity; for during my stay there the temperature of the air was only 38°.7, so that but very little could have melted away even from the upper surface. The water which trickled down from the surface of the glacier was also very inconsiderable.

I learned from my guide, a well-informed chamois hunter from the *Baths of Leuck*, that the water of this glacier-stream decreases as the seasons advance, and that in winter no more water flows out of the glacier. The same is the case with two small brooks, which fall in cascades from a small glacier, stretching down from the *Daubenhorn.* There can be no doubt that these also only result from the melting of the ice on the upper surface of the glacier by the heat of the atmosphere.

My guide also related to me some remarkable facts concerning the ice-cold, periodical spring of the *Leibfraw*, mentioned by Ebel,[*] situated 200 paces distant from the *Baths of Leuck*, which are also corroborative of the above opinions; namely, can take place, the original temperature of the soil may be somewhat raised. At such elevations, therefore, where the mean temperature of the soil is 32°, glaciers can only be melted on their upper surface, and during the warmer seasons, but in the winter no water can possibly flow from them.

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[*] Anleitung die Schweiz zŭ bereisen Zürich, 1810. 3d edition. Part iii. p. 333.

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My guide also related to me some remarkable facts concerning the ice-cold, periodical spring of the *Leibfraw*, mentioned by Ebel,* situated 200 paces distant from the *Baths of Leuck*, which are also corroborative of the above opinions; namely, when in the spring, water falls from the *Lötsch glacier* over a certain rock, which he pointed out to me, three days afterwards it flows out at that periodical spring, which has five outlets, all situated very near together, and, indeed, in such abundance that it might be used for driving a mill. This usually takes place in June; however, it depends upon the earlier or later commencement of the summer. These springs are also more copious the warmer the summer is. When the water ceases flowing over the rocks, the springs also, after three days, disappear, and this usually happens at the end of August, or the beginning of September. During my stay at *Leuckerbad*, the 3d and 4th of September, the spring only flowed from one of its onlets, and I learned from an engineer of the place, that the others had only ceased flowing a few days before. And, on the 5th, I observed no more water running over the rock. I have the less reason to doubt the information of my guide, as the low temperature of the spring (41°.4) can only be accounted for by supposing it to come either from a great height or out of a glacier. In the latter case, it can only proceed from the melting of the ice from the upper surface of the glacier during the summer. According to Hugi the lower extremity of the Lötsch glacier lies 5802 feet above the level of the sea, and consequently, according to Chap. XVIII., at an elevation where the mean temperature of the soil is 32°.0.

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* Anleitung die Schweiz zŭ bereisen Zürich, 1810. 3d edition. Part iii. p. 333.

It may therefore be assumed, that the glaciers in the *Alps*, which lie more than 6165 feet above the sea, are no longer melted away from underneath, by the internal heat of the earth; but that they are melted only on the upper surface by the heat of summer, and by rains.* Thus, on the upper surface, a layer of ice, although, *perhaps*, a somewhat thin one, will be formed, under which there will be nothing but soft snow to an unknown depth.† The thickness of this covering of ice will, of course, depend on the depth to which the water, resulting from the melting of the snow during the day, sinks and freezes during the night.

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* On the heights up to which the yearly fall of snow is melted away again, see Escher, p. 123.

† Von Buch in Gilbert's Annal., vol. xli. 15. De Saussure, Voyages, ii. 214, § 530.

If we trace a glacier from its lower to its upper end, we find that the crystals of ice become continually harder, till, at a certain height, it consists of nothing but a loose granular mass of snow, which, in *Switzerland*, is called *Firn*.* The height at which this *Firn* first appears, and which Hugi proposed to call the *Firn-line (Firnlinie)* is, according to his observations in the *Alps*, a fixed line, which undergoes neither elevation nor depression, whether on a northern or southern declivity, or from any other cause. He found the *Firn-line*, from observations on a great number of glaciers, to lie between 7614 and 7698 feet above the level of the sea. In the *Pennine Alps*, the *Firn-line* seems to lie somewhat higher, at least the observations on the *Gries*, and on the ridges of the *Binnenthal*, give it at near 7800 feet. Glaciers properly so called do not reach this *Firn-line*; for, at the height of 7600 feet, where the mean temperature, according to Chap. XVIII., is about 36°.5, the glaciers of the *Alps* are quickly converted into *Firn*.†

All these circumstances may be satisfactorily explained, if we consider that, where the mean temperature of the soil becomes 32°.0, the glacier no longer melts away from underneath, but only on its upper surface, during the summer. The greater number of the glaciers in the *Alps* extend far below the limit of 6165 feet, and consequently rest upon a soil, the mean temperature of which, if not covered with ice and snow, would be above 32°. It is this excess of heat above 32° which may possibly be employed in promoting a continual melting of the glaciers from underneath. Theoretical considerations bring us to the following conclusions:—The covering of an alpine valley with a glacier, causes a disturbance in the original law of the progressive decrease of heat from the centre of the earth towards the surface, which cannot be repaired so long as the covering remains; but the law is not entirely destroyed A distinction must therefore be made between the actual temperature of the soil beneath the glacier, and that which it would otherwise have, considering only its latitude and elevation above the sea. The difference between these two is the expression in degrees of the thermometer, for the quantities of heat communicated from the earth to the glacier lying upon it. The quantity of ice melted from the under surface of a glacier, in a certain time, would give an exact measure for the quantity of heat radiated from the earth. But we have no means of ascertaining that quantity with any degree of certainty. It is well known that stones, wood, corpses, &c. which fall into holes and fissures in the ice, but do not reach the bottom of the glacier, are again brought to the surface by the melting of the ice on its surface.[*]

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* Hugi in Berghaus Annal, iii., 295, Some of the assertions of this diligent explorer of the Alps, may perhaps be found to require some corrections, when subjected to a closer investigation. It certainly is not true that the masses which are converted into glacier ice lie deeper the higher the glacier lies above the *Firn*. I. 1. p. 294.

† I. 1. P. 289.

In the year 1828, Hugi buried several stones from ten to twelve feet deep in a glacier, and covered them in with ice, and the following year they were all on the surface again. This would give a measure for the quantity of ice melted away from the upper surface, and the absolute sinking of the stones would yield a measure for the melting of its under surface, did not the movement of the glaciers (the openings of the fissures), &c. occasion frequent changes in the position of the stones,[†] and if the angle which the under surface of the glaciers makes with the horizon were not unknown.[‡]

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[*] The common people in the *Alps* say, that the glacier will not suffer any foreign matter to remain in its interior. And, in fact, I have in vain endeavoured to discover even a single stone in the exposed masses of ice at the extremity of the glaciers. This is very plainly exemplified at the upper *Grindelwald* glacier, where perpendicular walls of ice, of more than 100 feet in height, are exposed to view. See Bisselx, in Gilbert's Annal., vol. ixiv. p. 198. However, at the lower *Grindelwald* glacier, the walls of ice are seen to be traversed by stripes, coloured brownish by earth.

[†] Kämtz, in Schweigger—Seidel's Journal, vii. p. 261.

[‡] From Mr Stählin's daily observations of the height of the *Rhine* at *Basel*, continued from 1809 to 1820, Escher calculates (Gilb. Annal., lxx. 202) that the *Rhine* carries off one billion and 46,000 million cubic feet of water yearly out of *Switzerland*. Now, supposing only one-half of this quantity to proceed immediately from the glaciers, and that of the fifty square German miles, which are said by Ebel (Part iii. p. 121) to be occupied by the glaciers of the Alps between *Mont Blanc* and the frontiers of *Tyrol*, only twenty square miles are tributary to the *Rhine* ; then, if the specific gravity of the water be considered equal to that of the ice of the glaciers, we find that, on an average, a mass of ice of fifty feet in thickness is melted away yearly from

Although it was to be expected *a priori* that the temperature of the soil under glaciers, however low they might extend, would never be much above 32°.0, yet I was desirous of satisfying myself on this point by direct observations.

Very near the lower extremity of the lower glacier of *Grindelwald*, on the 25th August 1835, I bored a hole six inches deep into the ground, and placed a thermometer in it. It marked 36°.5. At the upper glacier I found a temperature of 39°.8 twelve inches deep in the ground, as near to the ice as possible, but separated from it by the glacier stream, called the *Black Lutschine.* That neither of these temperatures was as low as 32°.0 is not to be wondered at; for, although they were observed as near as possible to the ice, yet these points were exposed to the air, which, at the lower glacier, had a temperature of 39°.8, and at the upper 45°.5. How trifling the distance is to which the coldness of the ice and the water issuing from the glaciers is felt, is proved by the latter of these experiments. The hole was bored so near the bank of the *Lutschine,* that its waters, of 33°.3, almost washed over the thermometer, yet did not quite communicate with the hole; and, notwithstanding this small distance, there was a difference between them of 6°.53. In like manner I found a difference of 8°.90 between the temperature of the earth and that of the stream, immediately on the bank of the *(Schwarze Lutschine) Black Lutschen,* near the bridge, between *Grindelwald* and the lower glacier, and at a distance of only eight paces, the difference was 14°.40. There can, therefore, be no doubt, that an observation made directly under the ice, would give a temperature very near 32°.0. I say very near zero, for the temperature of the soil under glaciers is seldom exactly 32°, since the streams that issue from glaciers, at the very moment of their exit, invariably shew a temperature a little above 32°. For example, six observations on the streams which issue from the lower glacier, at *Grindelwald,* all gave me a tem-

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those glaciers. Those natural philosophers who reside in the *Alps,* and are able to make their estimates more exactly, may easily make the necessary corrections in this calculation.

[*] Even if the stony nature of the soil under the glacier had allowed of such an experiment, the result would have been equally undecisive, as it would hardly have been possible to prevent the infiltration of water into the hole.

perature of 32°.9, another very small stream shewed 32°.7. The brook which flows from the upper glacier, gave a temperature of 33°.3, and that of the *Lammern* 32°.4. As my thermometers shewed exactly 32°.0 upon being plunged into a hole in the melting glacier, it is certain that they were correctly graduated.

There are three causes to which this elevation of temperature may be ascribed; firstly, the warm air drawing into the ice-caverns from which these glacier-streams issue, may tend to raise the temperature of their waters. This may be the case especially in the lower *Grindelwald* glacier; in a less degree in the upper *Grindelwald* and *Lammern* glaciers, because in neither of them does the water issue from caverns in the ice, but flows immediately from under the ice; secondly, the heat of the earth may assist in warming them; and thirdly, springs and streams of water, coming down from the mountains between which the glacier is enclosed, and flowing underneath the glacier, may also assist in raising their temperature. This seems indeed to be the case with the upper *Grindelwald* glacier, and may account for the high temperature of 33°.3 of the *Schwartze Lutschine*; namely, from the *Wetterhorn* there descends a considerable brook, the *Weisbach*—and from the *Mettenberg* a still larger one, called the *Milchbach*, both of which penetrate into the glacier, the one on the east, the other on the western side. Although these streams are nothing else than glacier-streams, proceeding from the upper parts of the glacier, yet during their course above ground they will acquire, in the warm seasons, a higher temperature, which they will afterwards communicate to the other waters with which they unite beneath the glacier. That there are currents of air between the descending stream of water and the ice, notwithstanding that there is no cavern in this glacier, is proved by the well-known adventure of the inn-keeper of *Grindelwald, Christian Boren*, who fell into a *crack* 354 feet deep in this glacier, crawled up 396 paces on his belly in the bed of the *Weisbach*, which he was three and a half hours in accomplishing, and came out on the *Wetterhorn*, where the Weisbach enters the glacier, having sustained no further injury than a broken arm. His son told me that his coat buttons were all rubbed off except the two top ones.

The canal was therefore very narrow, but yet, of course, filled with air.

That the above causes tend in most cases to raise the temperature of the waters which issue from them, is also proved by the observations made at my request by my friend Professor Ennemoser in *Tyrol*, in the summer of 1833. At ten different parts of six glaciers, he found the temperature of the water to rise as high as 34°.2. The large brook, which issues from the *Pfelderer* glacier, shewed at its first appearance a temperature of 35°.8. This high temperature was probably caused by this glacier having a southern aspect, and by the cavern out of which the stream flows extending itself very far beneath the glacier; for Ennemoser succeeded in penetrating to a great distance into the cavern, along the bed of the brook, and saw that the cavern extended much farther. And as the observation was made in July, there is the more reason to believe that this stream was indebted for its high temperature to warm currents of air.

The waters which collect on the glaciers themselves, as well as those which lie stagnant in holes in the ice, I found from numerous observations in the *Eismeer* of the lower *Grindelwald* glacier, and on the *Lammern* glacier, to have a temperature always between — 32°.2 and 32°.0. Into one of these holes, which was 8½ feet deep, with almost perpendicular walls, and the opening of which measured 2½ square feet, I sunk a glass bottle, which had been previously filled with water, of the surface of the hole, nearly to the bottom. I enveloped the bottle in wood-shavings, in order to prevent the contact of the glass with the ice. After the bottle had remained there 65 minutes, I drew it quickly up, and found the temperature of the water to be + 32°.4 At the surface, the temperature in this hole had a temperature of exactly 32″.0, while some which ran slowly into it out of a crack, about one inch deep, gave a temperature of —32°.2. The water which had collected in another hole, six feet deep, showed a temperature of + 32°.4 already at its surface. These observations prove clearly, that water which lies stagnant in holes in the ice, is warmed at its surface by the contact with the air, and, being thus rendered heavier, sinks to the bottom. It is well known that Rumford* has endeavoured to account for the formation of these holes in the glaciers, as well as their becoming deeper during the warm seasons, by the sinking of the particles of water, which have been warmed by the warm air at the surface, into the ice, by which the ice is melted away in a perpendicular direction. The holes seem, however, to owe their first existence, as Ebel* observes, to stones, which become heated by the sun to a greater degree than the surrounding ice, melt away the ice around them, and thus penetrate into the glacier. And, in fact, whenever I have been able to see to the bottom of such holes, I have invariably discovered one large or several small stones. It is evident that these stones can only continue to sink deeper into the ice, so long as the summer lasts, for after the water in them has become frozen during the following winter, the conditions necessary for the further sinking of the stones in the ensuing summer can no longer exist. On the contrary, the melting of the ice from the surface, gradually brings the stones out again. This sinking and reappearing of the stones may be continually repeated, until they reach the lower extremity of the glacier, or are precipitated into clefts, which extend to the bottom of the glacier. In this manner the stones may, by the sliding of the glacier, be made to perform a journey *en échelons*.†

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* Gilbert's Annals, lxviii. 362.

As these holes in the ice never reach the bottom of the glacier, and as the water which falls into the clefts is never above 32°, it is not possible that the water, which filters down from the surface to the bottom of the glacier, should come there with a temperature higher than 32°.0, which they have when they issue in streams from the lower end of the glacier. We are, therefore, obliged to have recourse to the above mentioned sources of heat, in order to account for this high temperature.

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* *Loco citato*, part iii. p. 117.

† This seems to me to account very plainly for another phenomenon, namely, since the glacier consists of a mass of snow irregularly intermixed with frozen water, and is consequently granular opaque, and very different from ice formed by the freezing of stagnant water, we ought to be able to distinguish that ice which was formed by the freezing of the water in holes in the glacier, from the rest of the mass of the glacier. But Escher remarks, that not one of the philosophers who have explored the Alps has been able to make such a distinction. (See Gilbert's Annals, lxix. 118). It is, however, evident, that it must be equally impossible to find any of the ice formed in the clefts of the glaciers, at the lower extremity, as to find the stones, because, when these reappeared at the surface, the ice by which they had been covered must necessarily have been melted away.

But if, as was to have been expected, *a priori*, the temperature of the water precipitated into the clefts of the glacier, is 32°.0, and if, as our experiments seem to shew, the highest temperature which it assumes before its exit at their lower extremity, is 35°.9, then can the temperature of the soil beneath the glaciers nowhere be much higher than 32°.0. This only refers to the warmer seasons; but as beneath the glaciers the earth is protected from the cooling effects of radiation, as even a very considerable degree of cold in the atmosphere is not capable of penetrating through a mass of ice of several hundred feet in thickness, and as, during the winter, the cold is almost, or perhaps entirely, prevented from penetrating into the caverns in the ice, by their being choked up with snow and ice, the temperature of the soil under the glacier will, even in winter, hardly be reduced below 32°. The yearly mean temperature of the soil under glaciers is, therefore, without doubt 32°.

The lower extremity of the lower *Grindelwald* glacier, lies, according to my barometrical measurement, at an elevation of 2989 feet above the surface of the sea, in a region where the mean temperature of the soil is much above 32°.0. Up to the *Eismeer*, at the foot of the *Vischhörner*, the glacier rises to a height of 2140 feet. So that even the *Eismeer*, at a height of 5129 feet above the level of the sea, lies in a region where the mean temperature of the soil is above 32°.0. Up to this limit, then, the glacier may be melted away from underneath, but not above this.

In *Grindelwald*, 248 feet above the lower extremity of this glacier, I found the temperature of the soil one foot below the surface, on the 26th August, to be 56°.7; on the *Metterberg*, near the *Eismeer*, and on the same level with it, it was 45°.5. At the former place the mean temperature will, therefore, not be much above 43°.2, at the latter hardly 36°.5.

Glaciers may be indirectly melted away from underneath at the expense of the internal heat of the earth, when, in the higher regions of the glacier water descends through cracks and fissures deep into the earth, and, having there acquired a higher temperature, reappears beneath the glacier at a lower point. This will more especially take place when the glacier rests, as most of those in the high lands of *Bern* do, on very fractured limestone rocks, in which we so frequently see rivulets disappear in the upper regions and shew themselves again lower down. I have witnessed many examples of this in the valley of the *Lutschine* between *Interlacken* and *Grindelwald*, on the *Spital-Matte*, and on the *Gemmi*. With a little attention many more such examples might be discovered. Directly on the north side of the heap of debris, (Gandecke) of 77 feet high, near the upper *Grindelwald* glacier, close to the *Bergelbach*, I found four springs whose temperatures were from 37°.4 to 38°.2, and which, as they are situated 40 feet below the lower end of the glacier, certainly owe their existence to water melted from the glacier, which flows down through clefts and underneath the glacier, from the higher regions. The low temperature of these springs, together with the appearance of one on the heap of detritus itself, having a temperature of 47°.4, and which could, therefore, not proceed from the glacier, are a sufficient proof of this. In former times, according to Ebel,* in the year 1720, when the glaciers probably also covered these springs, the case now under consideration must have taken place.

In like manner, but in a higher degree, the melting of the glaciers from underneath, by means of the springs which descend from the lateral mountains, will take place, as we have before said, when they rest upon a rock, which is almost or entirely free from fissures. On the road from *Grindelwald* to the *Eismeer*, on the eastern declivity of the *Mettenberg*, I found more than 14 springs and brooks, which altogether carried off a very considerable body of water. Their temperatures varied between 42°.01 and 49°.3, and their mean was 45°.83. It is true that several of these waters only proceeded from the rain and snow, which had fallen during the last few days, and that only a few of them fell under the glacier; but there were several others, of which even the oldest came down under the glacier, between the ice and the side of the mountain, with a temperature of 42.°01. It cannot be doubted that, even in winter, the springs which rise in this space must flow under the glacier.

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* III. p. 173.

As water of 41°, according to Rumford's[*] experiments, when left for 30 minutes in contact with ice of 32°, melts away about $\frac{1}{27}$ of its own weight from the ice, and becomes 3° colder, it will melt about one-eighth of its weight by the time that it reaches 32°. This will give an approximate measure of the increase of a spring, of the above temperature, which flows under a glacier.

The springs at *Leuckerbad*,[†] which rise in such enormous quantities, and with a temperature between 115°.2 and 124°.0 at a height of 4295 feet above the level of the sea, render it very probable that hot springs may also come up under glaciers.[‡]

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[*] Gilbert's Annal. vol. i. p. 334.

[†] Besides the *large* or *Lorence* spring, which rises like a brook in the front of the Bath-house, and is the warmest, I found twelve others at a little distance from the village, on the north-east side, which are all unused except two, and run into the *Dala*.

[‡] Many a warm spring may perhaps lie concealed in the deep valleys of the Alps. And as, in general, such springs rise according to the laws of hydrostatics, at the lowest part of the valley, we must expect to meet with them in the bed of the glacier streams. But there they can only be seen during the winter. Thus, in the winter of 1831, some fishermen for the first time discovered a warm spring of 113° in the bed of the *Rhone*, near the village of *Lavey*, to the south-east of *Bex*, which the government of the canton *Waadt* caused to be enclosed, and which has been raised since then to a watering-place. (See Ann. de Chim. et de Phys. vol. lviii. p. 109, and Journ. für Pract. Chemie, vol. ii. p. 82.) The fissures certainly descend to a great depth in some places into the interior of the rocks. This is evident from the water which oozes out at the foot of the high and naked cliffs of limestone. A most striking example of this is presented by the mountain over which the wonderful pass of the *Gemmi* leads, and which rises perpendicularly to the height of about 2000 feet. From all points of this cliff, from top to bottom, drops of water are seen to exude, and for the most part to unite below into a small brook. As when I crossed this pass, it had not rained during the last five days, this water must have come out of the interior of the rock. This is also very strongly corroborated, by the fact that the *Daubinsee* on the *Gemmi*, which measures one-half a league in length, has no visible outlet, although it receives the water which comes from the great *Lammerr* glacier, as well as all the rain and snow which falls upon the surrounding high mountains; further, that several small streams which come from the *Daubenhorn* gradually lose themselves in the earth; and, lastly, that on the *Gemmi* (as in the limestone rocks of *Crain*, especially near *Triste* and *Fiume*; see Gilb. Ann. vol. xlvi. p. 408, and on the *Wurtemberg Alp*, see Kastner's Archiv. v. 34) are found funnel-shaped holes, into which the rain-waters disappear. But waters thus sinking here and in other places through high mountains into the interior, will become heated, and will return to the surface in the valleys as warm springs. Examples of

The lower extremities of the two *Grindelwald* glaciers lie respectively 1306 and 571 feet lower than *Leuckerbad*. But warm springs may be met with at a still greater height than *Leuckerbad*. (See Chap. XVII.)

If we suppose the melting of the glaciers from underneath to be a consequence of the internal heat of the earth, we must also imagine it to be totally independent of the seasons, and that it takes place in winter as well as in summer. Saussure,* Escher,† Ebel,‡ and others, inform us that the glacier streams flow also in winter; however, the latter observes that they are less copious in that season.

Although the testimony of such philosophers as these, resident in the Alps, and gifted with such talent for observation, would be sufficient, yet I endeavoured during my stay in the Alps to obtain information myself upon this subject. All the inhabitants of *Grindelwald*, of whom I made my inquiries, agreed in assuring me that the lower glacier does not yield a single drop of water in the winter, but that the *upper one* continues to flow uninterruptedly. Supposing this information to be correct, I thought this difference might be explained by supposing that, as the brook which issues from the upper glacier has a temperature 4°.50 higher than that of the lower, its flowing uninterruptedly might be due to springs rising beneath the glacier. The chamois hunter I before mentioned, told me that the *Dala*, near *Leuckerbad*, which springs out of a glacier hanging down from *Balmhorn*, and also the *Massa*, which issues from the great *Aletsch*, a glacier extending from 9 to 11 leagues down from the south side of the *Jungfrau*, and terminating within two leagues of the *Rhone*, flow throughout the winter. But the upper *Lotsch* glacier, as I have already observed, does not yield a drop of water during the winter. In like manner, Ennemoser was informed that the *Pfelderer* glacier in *Tyrol* runs in winter as well as in summer.

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fissures extending to a great depth, and which are particularly frequent in the limestones, are to be found in the *Jura*, as for instance is proved by the *Orbe* spring, which is nothing else than the outlet of the lakes in the valley of *Joux*, which lie 680 feet above it.—Voyages dans les Alpes, vol. i. p. 309. Ebel, part iii. p. 221 and 586.

* Voyages, i. 453.

† Gilb. Annal. vol. lxix. p. 123.

‡ Ibid. iii. 121.

The scientific zeal of Mr Ziegler, the pastor of *Grindelwald*, and the observations so readily undertaken by him, and to which we shall return in Chap. XVIII. have enabled me to add to the above something more satisfactory. To his observations on the temperature of the soil, made at the depth of four feet near the lower glacier of *Grindelwald*, Mr Ziegler added observations on the melting of the glacier itself during the last winter. The former will not render us any assistance until next September, when the yearly series will be finished, but the latter has already yielded some very valuable results.

Temperature of the Soil.

1835,

Sept. 23. 39°.1.

Oct. 27. 35°.8. The temperature of the air inside the ice-grotto, close to the ice was 37°.8. The glacier melted away almost imperceptibly. The water which issued from the cavern was perfectly clear.

Dec. 2. 33°.3. South-west wind (called in *Switzerland* Föhn); the temperature of the air close to the glacier was 45°.5; the glacier melted away.

Dec. 28. 32°.7. The temperature of the *Schawrze Lutschine* under the ice, near the *Mettenberg* Bridge, was 32°.2. The severe cold, which continued through the whole of December, began on the 6th. From this time no more melting of the glacier could be perceived; indeed the water, which, on the 2d December, and perhaps for a few days afterwards, was clear and of a greenish colour, and which probably proceeded, as is the general opinion of the inhabitants of the place, from the brooks and springs, which throw themselves into the glacier above, was frozen to the bottom. This was observed, not only in a hole under the ice, which the inhabitants of a neighbouring cottage had made in the first covering of ice for the sake of obtaining water, but also in the ice-grotto itself, where Mr Ziegler had the ice broken up with a pick-axe, in order to ascertain the fact.

1836,

March 1. 32°.2. About the middle of February a rapid thaw took place, which lasted several days, and some persons assured Mr Ziegler that water ran from the glacier during that time; but it soon froze again to the bottom, and on the 1st March not a trace of water was to be seen.

April 4. 33°.1. Still no water made its appearance; and even on the 14th of April, the covering of ice and snow over the glacier stream was not broken, although there were frequent thaws throughout the month of March, and in the first half of April there fell a great deal of rain mixed with snow. Mr Ziegler, however, suspected that the water would soon burst violently forth, and overflow its customary channel.

These observations of Mr Ziegler's corroborate the information obtained from the inhabitants of *Grindelwald*. They prove that, in the winter, no more ice is melted away from the lower end of the glacier. And this was no more than was to be expected, as the freezing temperature to which the soil beneath the lower end of the glacier is exposed in winter, must necessarily cool it down to the freezing point, or still lower. These observations also prove, that even at the depth of four feet, the earth was reduced to the freezing point, so that at a less depth the temperature must certainly have been below 32°. But from this it by no means follows, that the temperature of the soil under the glacier, at a distance from its lower extremity, where it is not exposed to the immediate influence of the frost, is reduced to so low a degree. We must, therefore, still allow of the possibility of the melting away of the glacier from underneath, at the expense of the internal heat of the earth in those places. In this case, however, the water cannot effect its escape from under the glacier, because so soon as it approaches the lower extremity, it must freeze, and form such a dam of ice as to prevent all possibility of escape.

If it is as certain that some glaciers yield water in the winter, as that others give none, there seems scarcely any other mode of accounting for this difference, than by supposing the melting of the under surface of the glacier in the latter case only to be effected at the expense of the internal heat of the earth, and in the former by the additional assistance of springs, by means of which the water, even during the winter, acquires a temperature higher, though perhaps but by a few tenths of a degree, than the freezing point, which renders the uninterrupted efflux possible.

But where do the waters remain which in winter cease to flow from under the glaciers? Ebel[*] relates, that, in winter, nothing is to be seen of the caverns in the ice at the lower end of the glaciers: they are covered with snow and ice, but in the spring and summer the swollen streams break through the ice, and form caverns of 100 feet in height, and from 50 to 80 feet broad, the shape and size of which are variable.

This swelling of the water might, indeed, be attributed to the spring rains, which re-open the fissures in the ice, which had been stopped up with snow during the winter, and carry down great quantities of water, which burst a passage through the choked caverns. But if the caverns should be thus burst open before the fissures had been cleared out by the rains, the passage could only have been forced by the water which had collected under the glacier during the winter.* But as in glaciers which rise to a considerable height, the lower extremity lies in a much warmer region than the clefts into which the waters are precipitated, it might be expected that the caverns would be *reopened* sooner than the clefts in the glacier. It is not beyond the power of human observation, in the spring, to ascertain the real fact concerning this phenomenon. Well informed chàmois hunters, who ascend the glaciers at almost all seasons, would be the most proper persons to undertake such observations.

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[*] Part iii. p. 120.

There is another method, which, if pursued with caution, would give some information respecting the melting of the glaciers from underneath,--namely, if we suppose the glacier streams only to proceed from three sources; first, from springs and streams which sink into the glacier in higher regions; secondly, from the melting of the ice during the summer months on the upper surface of the glaciers; and, thirdly, from the ice melted away from the under surface of the glaciers, their contents of fixed substances must be different in the different seasons. The quantity of fixed substances contained in the streams which fall in the course of a year into the glaciers, as well as in the springs which rise underneath them during the same period, may be considered as constant. The water resulting from the melting of the glaciers on their upper surface during the warm season, can contain scarcely a trace of fixed substances, and that only when the glacier is covered with heaps of rubbish (gufferlinien). However, as these waters are precipitated into the fissures in the ice, and thus unite beneath the glacier with those of the streams and springs, they are enabled to take up fixed particles from the rocks over which they pass. But it is hardly to be expected that they would dissolve any other substances than carbonate of lime, if the rocks contain that substance; for we cannot suspect the presence of any more soluble salts in the rocks, because if they had ever existed, they would have been washed away already long since. The quantity of carbonate of lime which they dissolve, must also be very inconsiderable, as they have no opportunity of absorbing carbonic acid, while the atmospheric waters which supply the springs are already partly impregnated with this acid, and are also supplied with it from the superficial soil through which they pass. The same train of reasoning is of course also applicable to the water melted from the under surface, whether at the expense of the internal heat of the earth, or by the heat of springs and streams of water; but with this difference, that in this water we may suspect the total absence of carbonic acid, and consequently the least possible power of dissolving the lime. So that the more melted ice is mixed with the streams and springs under the glacier, the more will their original contents of fixed substances, and especially of very soluble salts, be diminished; and thus we may expect to find the smallest proportion of fixed substances in the streams which issue from under the glaciers, in summer, and the greatest proportion in the season when the melting of the glaciers almost or entirely ceases. From this we see that, by analyzing the water of the glacier-streams at various seasons, we may gain results, from which the quantity of ice melted away from the glacier may be inferred. If, for example, an analysis were made at the time when the temperature of the atmosphere is brought down just below 32°, the water could not possibly be mixed with any but that melted from the under surface of the glacier. If at the same time several neighbouring springs were also analyzed, and if the weakest among them still contained a greater proportion of fixed parts than was found in the glacier-stream, it might be concluded, with great probability, that the glacier still continued to be melted away on its under surface. In those glacier streams which flow throughout the winter, as, for example, that which issues from the upper *Grindelwald* glacier, the analyses might be continued in the severest frosty weather, and if it should be found always to contain less fixed parts than the neighbouring springs, it would be a proof that the glacier still continued to melt away from underneath; while on the other hand, if the quantity of fixed substances should be found to increase till it became equal to that in the springs, it might be inferred that the melting of the glacier had ceased.

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* Subterranean accumulations of water actually exist in the winter according to Ebel (part ii. p. 96), between the *Valsoré* glacier and the perpendicular wall of *Mont noir*, for a hole in that neighbourhood (called the *Gouille à Vassu*), 104 feet deep, remains from the autumn till July full of water, which in July breaks through under the *Valsoré* glacier, and forms ice caverns.

I confess that the conclusions, drawn from the results of such experiments, would only be within the range of probability. But what is left but probability in a field where absolute certainty is hardly possible to be attained?

An excellent spot for making these experiments would be *Grindelwald*, where in the winter one glacier, the upper one, always yields water, while the other, the lower one, gives none.

Mr Pagenstecher, the apothecary at *Bern*, had the goodness last autumn, at my request, to subject the water of the *Lutschine*, which issues from the lower glacier of *Grindelwald*, to a chemical examination, and communicated to me the following:

This water was taken from the cavern on a fine clear day near the end of October 1835, under the inspection and direction of Mr Zeigler, who sent it to Mr Pagenstecher with the following remarks. "At the time the water was taken, I ascertained that the glacier did not melt in the least. Last Sunday, when I also visited the glacier, it already melted but very inconsiderably, although the wind was from the south-west. On the bottom of the cavern wherever the rays of the sun did not penetrate, at this season the sun did shine on the lower end of this glacier only for one and a half hours during the day, I saw small icicles similar to those which are formed on the roofs of houses. It is, therefore, highly probable that the clear transparent water which now issues from the glacier proceeds only from springs; at least I cannot imagine how it could possibly be supposed to result even in part from the melting of the ice." When the water arrived at *Bern* it was also clear and colourless, and no trace of sediment would be perceived in the bottles. As the quantity of water evaporated and used for the analysis was as much as 658 ounces, Mr Pagenstecher was able to determine the ingredients, even in the most minute quantities. In 1000 parts of water the following ingredients were found:

Carbonate of lime, 0,40526; carbonate of magnesia, 0,01900;

silica, 0,06482; alumina, 0,00950; sulphate of soda, 0,07345; sulphate of lime with water of crystallisation, 0,30711; sulphate of magnesia, 0,14881; oxide of iron, a trace; organic matter, a trace; = 0,99795: Loss, 0,03102; = 1,02897.

No fluates, phosphates, nor strontian, could be found. Mr Pagenstecher had the goodness at the same time to undertake the analysis of the waters of the *Aar* and the *Rhine*. The former was taken from the middle of the stream near *Bern*, when the weather was fine and the water perfectly clear; and after twenty-four hours not the slightest trace of a sediment was to be perceived.

The *Rhine* water was taken near *Basel*, at some distance from the bank, also at a time when the river was perfectly clear. In the bottles in which it was sent there was no sediment, and the water was clear and colourless.

The analysis gave the following result.

	Aar.	Rhine.
Carbonate of Lime,	1,52191	1,27916
Carbonate of Magnesia,	0,16824	0,13511
Silica,	0,02693	0,02083
Sulphate of Soda,	0,00862	0,01845
Sulphate of Lime, with water of crystallisation,	0,17420	0,15357
Sulphate of Magnesia,	0,26005	0,03928
Chloride of Sodium,	...	...
Chloride of Potassium,	0,00287	0,01488
Alumina and Oxide of Iron,	a trace.	a trace.
Organic matter,	a trace.	0,03273
	2,16282	1,69401
Loss,	0,04992	0,01726
	2,21774	1,71127

If we compare the analyses of these three waters, we find a very near resemblance in their component parts: the only difference being, that in the *Lutschine* the chlorides are wanting which are found, although in but small quantities in both of the others. They do not, however, correspond so nearly in the quantities of e various parts; on the contrary, the difference is here very reat, for the *Lutschine* is distinguished by its far greater purity, at is to say, by its comparatively small contents of fixed substances. Yet it does not contain so little, as Mr Pagenstecher very justly remarks, as to lead us to suppose the water of the *Lutschine* to be only the product of the melting of the glacier, under which it takes its rise; but from the nature of its ingredients we should be inclined to consider it as entirely composed of the water of springs, or at least, with but a small admixture of glacier-water.

The comparison of the water of the *Lutschine* with that of the *Aar*, can, indeed, give no certain data, as the latter is formed by the union of so many different springs and glacier-streams; yet we cannot but be astonished to see that the quantity of fixed substances, contained in the water of the *Aar*, amounts to more than double that of the *Lutschine;* and that the quantity of carbonate of lime in the former, is almost four times as great as in the latter. It is true that the country tributary to the *Aar* is for the most part composed of limestones; but it is also true that the springs in the lower glacier of *Grindelwald*, as well as the glacier itself, are situated in similar rocks. Still it remains very remarkable, that the *Lutschine* should contain so little carbonate of lime; and this is certain, either that the streams which flow into the *Aar* and do not proceed from glaciers, must be very rich in carbonate of lime, for this river, after receiving into it the *Lutschine*, and so many other glacier streams, probably containing a proportion of carbonate of lime, equal to that of the *Lutschine*, is yet found to be so exceedingly rich in this earth; or that the *Aar* must dissolve a great deal of lime, during its course through the limestone mountains. The latter seems to be the more probable, since the *Aar*, as far as *Bern*, is supplied for the most part by waters proceeding from the glaciers. The smaller proportion of lime in the *Rhine*, at *Basel*, compared with that of the *Aar*, which shews that the *Rhine* must have contained a still smaller proportion previous to its union with the *Aar*, may perhaps be accounted for, by the whole of the country tributary to the *Rhine*, in the canton *Graübündten*, belonging to the primitive formation, except the northern part, which consists of clay-slate and lime.

None of the numerous fresh-water springs, which rise at the foot of the *Mettenberg*, and of the *Eiger* near *Grindelwald*, have yet been analyzed. However, I am in hopes that Mr Pagenstecher will add to his valuable experiments above mentioned, some experiments on some others of these springs, and then we shall be able to ascertain, whether the minimum of carbonate of lime in them is greater than in the water of the *Lutschine* or not. Not being in possession of such information, we will endeavour to answer this question beforehand, from other fresh-water springs rising out of the limestone rocks, and whose contents of carbonate of lime are known.

In 10,000 parts of a fresh-water spring, which rises out of the calcareous sinter near the *Laacher See*, I found 0.947 parts of carbonate of lime. In the *Pader*, which is formed by the union of a great number of fresh-water springs which rise out of the limestone at *Paderborn*, I found 2.5259 parts, and in the *Lippe*, which also rises out of the limestone, 2.265 parts of carbonate of lime. Schübler* found in the water, which issues from the limestone *quarries* of the *Wurtemberg Alp*, from 1.3 to 2.6 parts of lime, and in a fresh-water spring, which rises out of the same limestone, 2.2 parts. Several of the rivers of that country, as the *Neckar*, the *Ammer*, and others, gave him from 3.6 to 4.5 parts of carbonate of lime. All these waters contain, therefore, much more carbonate of lime, from twice to ten times as much, as the water of the *Lutschine*.† If, then, the springs, which rise also out of the limestone, and unite beneath the lower glacier at *Grindelwald*, contain at least so much carbonate of lime as the poorest of these springs, it would follow, that at the time when Pagenstecher analyzed the water of the *Lutschine*, it was mixed with more than an equal quantity of glacier-water. But as, according to Ziegler, the melting of the glacier could no longer be perceived, we may conclude, that the glacier continued nevertheless to melt away from underneath in its interior, where the cold of the atmosphere could not penetrate.

The currents of air in the channels in the glaciers may be compared with the ventilation in mines, for they are produced by the same cause, namely, the difference of density between the air within and the atmosphere.*

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* Kastner's Archiv. T. v. p. 1.

† Even in the *Laacher See*, a lake situated in a volcanic crater, and resting in some places on greywacke and clay-slate, far from any limestone rocks, found 0.4030 parts carbonate of lime in 10,000 parts of water, which is early as much as was found in the water of the *Lutschine*.

If the space through which a glacier stream flows, between the under surface of a glacier and the soil on which it rests, may be considered as one continued channel from the cleft B, (in Fig. 1, Plate 1, of next, or Vol. XXIV. of this Journal) to the ice-cavern A; then in summer, when the external temperature is above 32°, the temperature within being 32°, an uninterrupted current of air will issue from A. Hence the continual draught of cold air from the ice caverns, which is greater the higher the temperature of the atmosphere, and is consequently found to be more violent in the afternoon, growing weaker in the night, and almost ceasing towards break of day. Let us suppose, for example, as is the case with the lower glacier at *Grindelwald*, that B C = 2140 feet, let the temperature of the atmosphere be 86°.0 †, and that of the air in the channel beneath the glacier 32°, then the density of the column of air A G will be to the density of the column A B as 10 to 11. The velocity of the motion of the air in the channel depends on the difference of weight between these two columns of air, both supposed to be perpendicular. If we express the weights of the two columns of air of 2140 feet high, by the heights of two columns of water of respectively equal weights, we shall find—

For B C (column of cold air), . .	399.9 lines.
For A G (column of warm air), . .	363.6 lines.
Difference,	36.3 lines.

For one square foot of the section of the channel, or of the column of air, this difference would be equal to the weight of a quarter of a cubic foot of water, equal to 16,5 lb. Supposing the channel there to have no outlet from A to B, and to be wider than its lower opening in the same proportion that the tube of a large bellows bears to its contrated aperture; then, according to Koch's tables,* that difference would cause a current of air to issue from A, with a velocity of about 100 feet per second. But since the channels of the glaciers are not entirely enclosed from A to B, but have many minor outlets in the clefts D, E, F, H, and I, through which the air escapes in summer, and as they are not contracted at A, but, on the contrary, are enlarged at the lower opening in glaciers which have caverns, and as they have many unevennesses within, a great velocity cannot possibly be supposed to exist. From the smaller clefts D, E, and F, the air will issue in summer as out of A, but it will rush into the clefts H and I, which are situated higher up, in the same manner as it does into B. At a certain height, therefore, there must be a limit between the fissures through which the air enters and those by which it escapes; and this limit must be situated higher than the mean height B C of the glacier, when the upper fissures, and particularly B, are together larger than the lower ones together with the opening A; whilst, on the other hand, it must be situated below the mean, when the upper fissures are together smaller than the lower ones. From this limit upwards, the various currents will descend in summer more rapidly, and will escape more rapidly below, in proportion to their different heights.

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* I am indebted for the following calculation to the kindness of my friend and fellow-traveller Mr Althans, architect to the smelting works at *Sayn*, with whom I held frequent consultations, during our visits to several glaciers, on the various circumstances related above, and the results of which I have made use in preparing this chapter.

† Ebel, vol. i. p. 6.

Let us suppose the velocity of the currents to be but one-tenth of that found above, still the volume of warm air introduced is very considerable; and as they must be cooled down during their long course through the glacier, nearly to 32°, it may easily be conceived how great an influence such currents of air must have in melting the glacier away from underneath, as well as in raising the temperature of the glacier-streams.

If the ice-caverns become completely blocked up in the winter, the currents of air under the glacier will be entirely stopped. But even though the glacier be covered with snow, yet all the fissures will not be got closed up so as to prevent the passage of the air; for many places will still be left, through which currents of air, though perhaps but inconsiderable ones, will find a passage. But these currents will take a contrary direction in the winter, and will enter from below, and make their escape above, so long as the temperature of the atmosphere is lower than that of the interior of the glacier. However, these currents, which tend to cool the under surface of the glacier, can never be so considerable as those which take place in the summer, for the cold air cannot be warmed within the glacier above 32°, so that there can be but a small difference between the density of the atmosphere and that of the air within. The under surface of the glacier cannot, therefore, suffer any considerable depression of temperature during the winter, so that the hypothesis, that the soil beneath the glaciers remains at about 32° throughout the whole year remains almost unshaken.

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* Experiments and observations on the velocity and quantity of compressed mospheric air, which issues from apertures of various forms, and through bes, &c. Gottingen, 1824.

It is probable that the piercing cold currents of air, which issue from the fissures, when a sudden change takes place in the temperature of the atmosphere, carrying fine grains of ice along with them, and scattering them, far and wide, like fine snow over the country,* take place when the external temperature suddenly sinks below 32°.0, by which the current is found to alter its direction, and to rush from the cavern towards the fissures above. For if the air which issues from the fissures at a temperature of 32°, be impregnated with humidity, when it enters an atmosphere of a temperature below 32°, the water must be frozen, and thus form the grains of ice. And this phenomenon is precisely analogous to one which I observed a short time since at the mouth of a shaft, during a sharp frost, where the warm air, which rose out of the shaft, deposited flakes of snow on the whim, and on the buildings over the shaft.

It is of importance to the study of the internal heat of the earth, to ascertain whether glaciers in general increase or not. The annual advance of the glaciers during the warm season, which Saussure† very simply explained by the under surface be-

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* Ebel, part iii. 116. Gruner in his "Beschreibung der Eisgebirge des Schweitzerlandes." Bern, 1760, relates an example of this, witnessed by a friend of his, who travelled across a glacier in the middle of July.

† Voyages, i. p. 453. Escher in Gilb. Annal. vol. lxix. p. 113. Horner in the Neuesphysikal, Wörterbuch, vol. iii. p. 135. Against this opinion see

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coming slippery by the melting of the ice, and thus allowing the glacier to slide down the sloping declivity, is a well known fact. Whether this advance be greater than the loss sustained by the melting away of the ice, depends not only on the warmth of the summer, but also on the greater or less quantity of avalanches, which fall during the winter on the upper part of the glacier. And this latter condition again depends not only on the quantity of snow which has fallen in the winter, but also on the direction which the avalanches take. But this direction may be modified by various local circumstances, by the change in the slope of the side of the mountains, caused by the falling in of large masses of rock, &c. Escher further observes, that, in warmer years, the side edges of the more elevated glacier, are often melted to a greater extent, and that that part of the glacier which is situated at the entrance of a cross valley, is thus more easily carried forward, and is forced in greater masses into the valleys below. Thus it may often happen, that some arms of a glacier increase in length for several successive years, whilst others, situated quite near them, become shorter.* It must be remarked, that the glaciers in elevated valleys extend down both sides of the mountain, and thus two or more arms or branches are formed, which meet together on the top of the ridge.

If the lower part of a glacier, by being kept slippery by the melting of the ice from its under surface, or higher up, by the melting of the upper surface, and the penetrating of the water to the under surface, slip down, the upper part, which only consists of loose snow, without any cohesion, will follow like an avalanche, and thus come into a region, when the snow will be converted into ice.

Besides the increase and decrease of the glaciers, caused by the above mentioned circumstances, it is a pretty general opinion

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V. Charpentier in Gilb. Annal. vol. lxiii. p. 388, and Biselx in the same, p. 192. Kühn (Höpfner's Magazine, vol. i. p. 129), observed an advance of the lower *Grindelwald* glacier, accompanied with a violent subterranean noise, when he was on the *Eismeer*, by which several very wide fissures were suddenly closed, and the water which was in them forced up to a considerable height.

* Numerous examples of this are to be found in the Biblioth. Univ. xiv. tu.285.

in the Alps that on the whole the glaciers increase. Thus Ebel[*] mentions in several places that, according to tradition, many elevated valleys, which are now quite filled with glaciers, were formerly fruitful.[†]

Whether these traditions, themselves not sufficiently confirmed, are enough to prove the increase of the glaciers to be a general phenomenon, is very doubtful.[‡]

This, however, is certain, that there are two causes which operate uninterruptedly in the Alps, and act in opposition to the increase of the glaciers. These causes are the wearing away of the earth beneath the glaciers, caused by their progress downwards, and by the continual running of the streams under them; and, secondly, the falling down of ridges and of high masses of rock.

When we consider that there is always a great quantity of stones and blocks of rock beneath the glaciers,[§] which must sustain all the weight of the glaciers during their movement, it is easy to conceive that the rocks upon which they repose must be worn away by the friction, especially when the blocks are of a harder material than the rock.* On that view the glacier can be compared to a plane of a very extraordinary power. How very cleavable some of the rocks in the Alps are, is exemplified in the clay-slate of the *Faulhorn* and the *Schwarzhorn*, which colours the *Bergelbach*, on the upper *Grindelwald* glacier, black. In the bed of the *Schwarze Lutschine* I found large fragments of this slate, which might be crumbled to pieces in the hand. The separation of these blocks is certainly facilitated by their being kept continually wet under the glacier. The glacier streams carry these blocks away not only mechanically, but also by dissolving them, when they consist of lime.† In proof of this, Ebel‡ points out the extraordinary winding furrows, which are seen on the broad *Mosa Alp*, on the south side of the *Muschelhorn*, on the northern side of the *Gemmi*, and on the small plain of the *Rhine* glacier; and which he is the more inclined to take for the action of glacier streams, as the same furrows are sometimes observed in the rocks on which the *Rhine* glacier rests when in very hot summers any part of it is melted away. Besides the detrition of the rocks on which the glacier rest, we have also to take into consideration the deepening of the beds of glacier torrents. In a quarry on the road in the valley of *Frutigen*

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[*] Part II. 302; III. 173; IV. 109, &c.

[†] Buffon, Epoques de la Nature, 6ème edit. Note justif. 31.—Bourrit, des glaciers de Savoie, Génève, 1773, p. 111. The same in his Descrip. des aspects du Mont Blanc, Lausanne, 1776, p. 8. 62.—Bergman, physikal. Erdbeschreib. Part II. p. 5. c. 2. § 157.—Altmann's Versnch einer historisch phys. Beschreib. der Helvet. Eisgebirge. Zürich. 1751. 141.—Schultes ueber die Tyroler Gletscher in Gilb. Annal. xx. 243; von Buch on those of Norway in the same, vol. xli. p. 22.—Pontoppidan, Natürl. Historie v. Norwegen, i. 56.—Hansteen in the Edinburgh Philos. Journ. x. 213. Against this, see Naumann in Leonhard's Taschenbuch Jahrgang, xvii. p. 163.—Scoresby ueber die Gletscher in *Spitzbergen*. Gilbert's Annal. vol. lxix. p. 142.—De Luc in the same, p. 149.—Charpentier in the same, vol. lxiii. p. 408.—Biselx in the same, vol. lxiv. p. 191.—Pictet in the same, p. 200.—Anmerk the same, vol. lx. p. 334.

[‡] Von Welden (der Monte Rosa, Vienna, 1824, 78. 81;) Kasthofer (in his prize Essay in Zschocke Ueberlieferungen, 1820, 505 and 574;) Hegetschweiler (Reisen in den Gebirgsstock zwischen, Glarus, and Graubünden in den Jahren, 1819, 1820, and 1822, Zürich. 1825, pp. 11, 12, 41, 54, 103;) and Hugi (Naturhistorische Alpenreisen, Solothurn, 1831) mention indeed many examples of periodical advances and retreats of glaciers, but found no proofs of their absolute increase within thousands of years.—Venetz (see Kastner's Archiv fur Chemie und Météorologie, ii. 225) thinks himself entitled to conclude, from various geognostical phenomena, such as the occurrence of large loose blocks of rock on extensive plains, &c. &c. that the glaciers are in the act of retreating.

[§] The immense heaps of *detritus* (Gandecken, Moraines) which are found below the glaciers, as well as at their sides, are a proof of this; besides which, we are told by the innkeeper at *Grindelwald*, who has become so celebrated by

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the adventure related of him above, that the numerous blocks of stone which he met with in his extraordinary journey under a part of the upper *Grindelwald* glacier, were the greatest impediments to his progress, because they always left him in doubt whether he should crawl round them to the right or to the left.

* A wonderful example of the irresistible power of the glaciers when they are in motion is related by Kuhn (Höpfner's Magazine, f. d. Naturkunde Helvetiens 1787, i. 130.)

† The considerable beds of calcareous sinter, which are found in the environs of *Grindelwald*, and on the way from thence to the *Faulhorn*, are proofs of this. From the above mentioned observations of Stählin on the quantity of water which is annually carried out of *Switzerland* by the *Rhine*, and from Pagenstecher's analysis of the water of the *Rhine*, it may be calculated, supposing the specific gravity of the fixed substances to be equal to that of carbonate of lime, that a cube of solid matter of 866 feet side is annually carried out of *Switzerland* in solution alone.

‡ Part II. p. 256; III. 31; IV. 111.

near *Müllinen*, are to be seen among others the same rolled stones which are now carried along by the *Kander*, some hundred feet lower. To this action of the deepening of the beds of glacier streams must be added the immense pressure sustained under the glaciers.

Let us suppose, that in a certain time the destruction of the rocks under a glacier proceed to a depth of 115 feet, the glacier will thus be brought down into a region in which the mean temperature of the soil is 2°.25 higher than before. It will now, therefore, be melted more rapidly; and, if more is not supplied from above than before, it must suffer a decrease in size, nay, in a certain time it will disappear altogether. That many glaciers have actually decreased to a considerable extent in progress of time, is evident from the heaps of detritus (Gandecken), which are often found at a considerable distance from their present lower extremity.*

I am inclined to consider these evident signs of the former and present extent of glaciers, as more decided proofs of their decrease than the mere tradition of the formation of new glaciers is of their increase. However, I do not mean, by the above train of reasoning, altogether to deny that glaciers may also increase, and that new ones may sometimes be formed. It is only the universality of the phenomenon that I am calling into question. With regard to the second of the two causes given above, as acting in opposition to the increase of glaciers, viz. the falling in of high points of rocks, it is self-evident that glaciers must disappear, when the mountains covered with perennial snow, from which they are supplied, are destroyed. The hypothesis of Ebel,† that the northern side of the *Gemmi* to the *Dauben lake* was formerly covered with glaciers, is therefore certainly correct; for we have evidence of this in the enormous fallings-in which have there taken place, probably in consequence of the facility with which the slate on which the *Gemmi* rest is disintegrated, and hence the great heaps of detritus at the southern foot of the *Gemmi*, and in the valley of the *Rhone*. Traces of similar fallings-in of the rocks, and of extensive ancient glaciers, are also to be found on the northern side of the *Gemmi*, below the inn of *Schwarrbach*, and near the *Spital Matte*. On this Alp are seen several hills covered with fir-trees, and formed of blocks of limestone, disposed in irregular layers, one above another. Out of the innumerable clefts in these hills rise the numerous freshwater springs which we have already before mentioned, and which, after forming a considerable stream, sink into the rocks and reappear, together with several other springs at the foot of one of the smaller hills. Below *Kanderstäg*, on the left declivity of the mountain, we find a very large heap of detritus, which we are indeed inclined to consider as an enormous *Gandecke*. But even further down, as far as *Frutigen*, we see considerable hills composed of blocks of limestone heaped together, sometimes in the middle of the valley, and sometimes resting on the high mountains on each side. The luxuriant vegetation in the valley shews, as every where, that the soil here rests upon fragments broken off and precipitated down from the rocks above.* Traces of such dislocations are well known to exist in the Alps, and the history of recent times furnishes many melancholy examples of the same. Thus, for example, the sudden fall of the rocks, and the destruction of the city of *Plurs* or *Piuri* † in the year 1618,—that of the village of *St Abundio* ‡ in 1760,—the falling-in of the *Diablerets* in the years 1714 and 1749,§—that of the *Ruffi* or *Rigi* at various periods,—and the latest and most dreadful catastrophe of all, which filled the valley of *Goldau* with rubbish in 1806.‖

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* For the sake of example, we may mention the Gandecke, already cited above, near the upper *Grindelwald* glacier; that of the lower glacier, which is now covered with trees; as well as that of the present *Rhone* glacier, which lies at a distance of 2404 paces from it. May not the heaps of detritus which are found in valleys at a great distance from any glacier, also have been formerly Gandecken?

† Part III. pp. 30 and 338.

Such disturbances must be frequently repeated, as the rapid streams below undermine the feet of the mountains, by which the rocks are made to split and to sink, the fissures sometimes extending more than a thousand feet above the bottom of the valley; into these the waters enter in the following spring, soften the rocks, and thus cause their fall.*

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* Escher in Gilbert's Annal. vol. lx. p. 363.

† Ebel, part ii. p. 365.

‡ Ibid. p. 367.

§ Ibid. p. 446.

‖ Ibid. p. 146.

The alternation of frost and thaw also causes the dislocation, and breaking done of considerable masses of rock. Thus Gérard observed in the *Himalaya* mountains, that masses of an enormous size were continually falling down from some rocks, between which there were passes, at a height of 15,000 to 16,000 feet. The bottom of the passes was covered with immense heaps of such fragments.†

But the universal consequence of such fallings in of the rocks will be, that the glaciers will lose their supply of snow, and will decrease, or perhaps entirely disappear. Thus by degrees many spots are reclaimed from the regions of perennial snow, and become capable of sustaining organic life.

CHAP. X.—*The frozen soil of northern Siberia is no contradiction to the increase of temperature towards the interior of the earth.*

The whole of northern *Siberia* presents the singular phenomenon, that, even in the hottest season, the soil remains frozen from a certain depth downwards, differing according to the latitude, and other local circumstances, and that the thickness of this frozen stratum is so considerable in the more easterly places, as, for instance, at *Jakutzk*, that its bottom has not yet been reached.‡ Gmelin relates that in the archives at *Jakutzk*, he found an account of an inhabitant of that town having, at the beginning of the last century, together with some Jakuters, contracted to sink a well, and that when they had reached the depth of 90 feet, finding the earth still frozen, they refused to fulfil their engagement.*

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* We have proofs of the decrease of the height of the Alps in former times, in the loose blocks of rock which lie scattered about in all directions, as it seems certain, from the observations hitherto made, that they have been precipitated from the upper Alps. Die Umwalzungen der Erdrinde von Cuvier, translated by Noggerath, 1830. ii. p. 15. It is also proved by the deposits from the alpine streams in places where their course is less rapid, by which means enormous masses have been and still continue to be carried out of the the Alps.

† N. Geograph. Ephemeriden. xiv. p. 262 and 276.

‡ To this phenomenon is to be ascribed the existence and the good preservation of the great tropical animals, covered with their flesh and other soft parts, found at the mouth of the *Lena*, and on the banks of the *Wilhui* in latitudes 72° and 64°.

Some philosophers have considered this contradictory to the supposition that the interior of the earth is in the state of fusion. But from the following account it will be seen that, in those frozen strata, the general phenomenon of an increase of temperature with the depth is not wanting, and that by continuing the work, they have arrived at a temperature which leaves no doubt that they are not far from the lower limits of the frozen soil, and that water, the object of their undertaking, is not far distant.

It is well known, says an article from *St Petersburg*, in the Berlin news of the 24th February 1832, that at *Jakutzk* in *Siberia*, the earth, even in the hottest summer, only thaws to about the depth of three feet. Hitherto all attempts to discover the thickness of the frozen strata beneath, have been fruitless. Since the year 1830, one of the inhabitants of *Jakutzk* has been engaged in sinking a well, by which means it may, perhaps, be ascertained. In the same year the workmen reached the depth of seventy-eight feet below the surface, but still found no water. In the year 1831, they reached ninety feet, and were still in the frozen soil. The work is still in progress, and there seems no doubt of their attaining their object, for the thermometer, which shewed 18°.5, a few feet below the surface, rises, when sunk to the bottom of the well, to 29°.75.†

It is evident that in high latitudes, where the mean temperature of the soil is below 32°, there must exist a frozen stratum of earth of a certain thickness, which can only thaw during the summer, to that depth beyond which the influence of the external temperature is no longer felt. For, in the same manner as in our climates, the soil is frozen to a certain depth by the cold of winter, so must the frozen soil, in those high latitudes, be thawed to a certain depth by the heat of summer, In *Siberia*, the temperature of the soil is near 32°, in the latitudes of the *North of England* and *Scotland;* for Von Humboldt and Adolphus Erman found wells and springs of 33°.3 to 34°.4 in the months of July and August, in such latitudes. At *Jakutzk*, therefore, in 62° North Lat., the mean temperature of the soil must already be considerably below 32°. Thus, if we suppose it to be only 27°, and that the temperature increases towards the centre of the earth at the same rate as it has been found to do in our climates, the mean temperature of the earth would not reach the melting point of snow until a depth of 230 to 256 feet.

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* Against this, see V. Buch in Poggendorff's Annal, vol. xii. p. 405.

† See Erman, in Poggendorff's Annal., vol. xvii. p. 340, Von Humboldt, ibid. vol. xxiii. p. 104 and 106, and Hansteen, ibid. vol. xxviii. p. 584.

Chap. XI.—*The decrease of Temperature in the Waters of the Sea and of Lakes, is not contradictory to the hypothesis of an increase of Temperature towards the Centre of the Earth. On the contrary, we can only explain the Temperature of Sea and of Lakes by admitting an increase of Temperature towards the Centre of the Earth.*

De Saussure * determined the temperature of the *Swiss Lakes* at various depths, as in the following table:—

Places.	Temperature of the Water		Depth in feet.	Dates.
	on the Surface.	at the Bottom.		
Lake of Geneva, ...	42°.09	41°.74	950	1777, Feb. 6.
Do. do.	70.13	43.09	150	1775, Aug. 5.
Lake of Thun, ...	65.5	41.00	350	1783, July 7.
Lake of Brienz, ...	66.24	40.69	500	1783, July 8.
Lake of Lucerne, ...	68.49	40.74	600	— July 28.
Lake of Boden, ...	63.59	40.00	370	1784, July 25.
Lago Maggiore, ...	77.00	44.03	335	1783, July 19.
Lake of Neufchatel, . . .	73.59	41.00	325	1779, July 17.
Lake of Biel, ...	69.23	44.34	217	1779, July 20.
Lake of Annecy, ...	57.84	42.09	163	1780, May 14.
Lake of the Bourget, . . .	63.24	42.09	240	1781, Oct. 6.

According to De la Beche's observations on the *Lake of Geneva*,† the temperature of the water, at depths of forty to seventy fathoms, seems everywhere to be 43°.9, except only at *Ouchy*; and from eighty fathoms downwards to the greatest depths, he found a constant temperature of 43°.04. In lesser depths than forty fathoms, the temperature varies according to circumstances, but to that limit it always decreases from the surface downwards. In the *Lake of Thun*, De la Beche found the temperature at the surface to be 59°.9, whilst at a depth of 105 fathoms it was 41°.4; and in the *Lake of Zug* the temperature on the surface was 58°, whilst at a depth of thirty-eight fathoms it was 41°. Three hundred to four hundred fathoms deep in the *Lagodi Como*, Volta found a temperature of 43°.2.[*] On the 16th April 1798, Von Humboldt found the temperature of the water of the *Lake of Barthomew*, in the *Berchtesgaden Alps*, at a depth of 2 feet, to be 45°.83; at 42 feet, 43°.13; at 60 feet, 41°; and at another spot, at a depth of 84 feet, 42°.09.[†] Recent measurements of D'Urville,[‡] and of Boubée in the *Lake of Oo*, near *Bagnères de la Chou*,[§] have given the same results.

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* Voyages aux Alpes, § 1351 and 1391.

† Gilbert's Annalen, vol. lxvi. p. 146.

Thus we find that the temperature in the depths of lakes approaches that of fresh water at its greatest density, which, according to the latest accurate experiments of Stampfer, is 38°.7.[||]

On the temperature of the sea in various depths, numerous observations have been made, sometimes giving contradictory results, which may have been partly owing to the various methods and instruments used.

From more than fifty observations, made by Castberg [¶] on the temperature of the water, at depths varying up to 200 feet on the coast of the *Mediterranean*, he found no regular variations. Peron[**] concludes from his own observations, as well as from those of Irvine and Forster, 1*st*, That far from the coast, the temperature of the sea is lower at all depths than at the surface, and that it decreases with the depth, apparently according to a certain law; 2*d*, That this holds good, as well for the frozen seas of the polar regions, as for the burning climates under the equator; only that at equal depths, the cold is comparatively much greater near the poles than under the equator. But he goes further, and concludes from this, that the deepest abysses of the sea, as the summits of our mountains, are covered with perpetual ice, even under the equator, and therefore considers that so many facts are sufficient to overthrow the hypothesis, now so generally adopted, of the existence of fire in the centre of the earth.*

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[*] Gilbert's Annal. vol. ii. p. 402.

[†] Reise in die Acquin. Gegend. iii. 132.

[‡] Bibl. Univ. 1833, Nov. 323.

[§] London and Edinb. Phil. Mag. 1832, p. 383.

[||] Jahrbücher des K. K. Polytechnischen Instituts zu Wien, vol. xvi. p. 48.

[¶] Gilbert's Annal. vol. xix. p. 344.

[**] Annales du Museum d'Hist. Nat. t. v. p. 123 to 148, and Journ. de *Phys.* t. lix. p. 361. Also in Gilbert's Annalen, vol. xix. p. 427.

Von Buch† came forward in opposition to this last conclusion, and declared himself hostile to the formidable idea of the existence of such a crust of ice, which is by no means rendered necessary by observation. If, says this ingenious philosopher, the internal cold is felt so near the surface of the sea, should it not also be felt at those depths in the earth near the sea, which are so distant from the surface that the latter can no more have any heating effect on it? But has there ever been found in the wells of *Amsterdam*, which are sufficiently deep, a temperature sensibly lower than the mean temperature at the surface? Yet, notwithstanding the remarks communicated by Von Buch already thirty years ago, Parrot has lately again attempted to bring forward the decrease of temperature of the sea, and of lakes, in proportion to the depth, as an objection to the increase of temperature towards the centre of the earth.[*] But this objection has been silenced by Klöden.[†]

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* Peron (Annales du Museum d'Hist. Nat., vol. v. p. 123 to 148, and Journ. de Phys., vol. lix. p. 361) found several zoophytes, fished up from the bottom of the sea on the coast of *New Holland*, to be 6°.75 warmer than the air and the surface of the sea. He puts the question, Whether the zoophytes inhabiting the bottom of the sea live as the more perfect animals and plants do, in a warmth peculiar to themselves, exceeding, at least in some cases, that of the surrounding medium? It is a matter of doubt whether plants do possess a warmth of their own (see Treviranus, Biologie, vol. v. p. 3 and following). According to all observations, namely, those of Schübler (Poggendorff's Annal. vol. x. p. 581), this is by no means the case. Among animals, birds have the highest temperature; next to them the mammalia; then the amphibia, fish, certain insects; and lastly the molluscæ, crustacea, and the worms (John Davy in the Edinb. Phil. Journ., vol. xiii. p. 300, and vol. xiv. p. 38). But if, as J. Davy asserts (in a letter to H. Davy of the 18th May 1816, in the Journ. of Science), the temperature even of fish is only 2°.025 to 2°.700 higher than that of the water, (see in opposition to this Von Humboldt and Provençal in the Mem. de la Soc. d'Arc., vol. ii. p. 598), the temperature of the zoophytes must be scarcely perceptibly higher than that of the water. Should the zoophytes, notwithstanding, show a higher temperature, which, however, remains to be proved by more accurate observation, it ought rather to be supposed that such heat is derived from the bottom beneath, and that they retain it longer than the surrounding water does, which, by becoming specifically lighter, rises and carries its excess of heat to the surface, than that it is a heat peculiar to itself.

† Gilbert's Annal. vol. xx. p. 341.

The decrease of temperature in the sea, in proportion to the depth, has also been confirmed by Captains Ross and Sabine, as well as for the northern ocean by Lieutenant Parry.[‡] On the other hand, Lieutenant Franklin found the temperature sensibly higher, in high latitudes, at great depths in the *Greenland Sea*, than near the surface, and the difference was frequently as much as 9°.00 or 11°.25. Other officers of this expedition, Lieutenants Beechy and Fisher, observed the same. As most of Franklin's observations were made whilst the ship was surrounded with ice, the temperature of the surface of the sea must have been about the freezing point, which was probably the cause of his contradictory results.[§] These observations can, therefore, not be taken into consideration in the comparison of the temperature of the sea at its surface, and at various depths.

We are indebted for a valuable series of observations on the temperature of the sea at various depths, to an observer in whom the utmost reliance may be placed, namely, the astronomer who joined Krusenstern's Voyage of Discovery, the late Horner[‖] of *Zurüch*. He found that the temperature of the sea invariably diminishes as the depth increases. But single spots are to be found in the sea, where the temperature is greater in the depths than at the surface. Hot springs and volcanic action may cause considerable partial elevations of temperature in the sea, as, for example, seems to be the case in the *gulf stream* on the coast of *America*, where, on hauling up the lead from a depth of 80 or 100 fathoms, it is so hot as scarcely to allow of its being handled. Similar spots seem to exist near the *Kurile Islands*, in *Basse's Straits*, and in the *Atlantic Ocean* where Horner in clear weather observed, at about nine miles' distance from the vessel, a cloud of vapour, which, during a quarter of an hour, continued alternately appearing and disappearing from the surface of the sea, and could neither be the smoke of powder nor that of a vessel on fire. Horner considers that this phenomenon may perhaps have been caused by a volcanic eruption. In opposition to the opinion that the bottom of the sea must in very great depths consist of a solid mass of ice, Horner brings forward very conclusive arguments.

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[*] Von Leonhard und Bronn Jahrbücher der Mineralogie, Geognosie, Geologie, &c. 1830, fasc. 3. p. 334.

[†] Ibid. 1831, fasc. 4. p. 384.

[‡] Alex. Marcet's lecture before the Royal Society of London, 20th May 1819.

[§] See Gilbert's Annal. vol. xx. p. 254.

[‖] Ibidem, vol. lxiii. p. 266, and following.

Lastly, a series of observations were made by Lenz,* on a voyage round the world, under the command of Captain Von Kotzebue, in the years 1823 to 1826, with the utmost care and circumspection, so that particular confidence may be placed in them. They yielded the following results:—1st, Between 45° north latitude and the equator, the temperature of the ocean constantly decreases to the depth of 1000 toises, below which no experiments have been made. 2d, The decrease of temperature is at first rapid, but becomes slower and slower, and is at last scarcely perceptible. 3d, The point at which the decrease begins to be imperceptible, seems to be situated less and less deep the further we recede from the equator. Under latitudes 41° and 32°, its depth is 200 to 300 toises; in latitude 21°, it is 400 toises. 4th, The lowest temperature observed in great depths, was 35°.93, and this is about the temperature of all points where the decrease begins to be no longer perceptible.

According to the third mentioned result, this temperature is found nearer to the surface the higher the latitude; it would be interesting to ascertain in what latitude it reaches the surface.

We will now proceed to the consequences which may be deduced from the decrease of temperature in the sea and in lakes.

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* Poggendorff's Annal. vol. xx. p. 73 and following, extracted from a paper read by Lenz before the Academy of St Petersburg, on the 4th of November 1829.

From the above observations on the temperature of the lakes in *Switzerland*, which were made at various seasons of the year, we see that the temperature on the bottom approaches very near to the degree at which water assumes its greatest density; but never quite reaches it. It is always 1°.35 to 5°.74 higher than 38°.7. Suppose a lake in the autumn to have everywhere in its depth a temperature of 54°.5; and that the water neither gives nor receives heat from the earth on which it rests. Now, if cold set in, and the surface of the lake be depressed below 54°.5, the water at the surface becoming more dense, will sink to the bottom, and continue to do so until it be everywhere reduced to 38°.7. If the cooling of the surface continue still further, it will have no more influence on the temperature of the water below; for now the water at the bottom of the lake can only become colder by imparting its heat to the colder water above; which, however, cannot take place, water being so bad a conductor of heat, and the lake having so great a depth. The lake will therefore have a temperature of 38°.7 from the bottom, nearly to its surface. If the temperature at the surface then begin to rise again, the only increase which will be caused in the temperature of the water below, will be by the communication of its heat. But for the reason just mentioned, this cannot extend far; and, in fact, the observations of De la Beche gave equal temperatures at depths of 40 to 70 fathoms; and also at depths of 80 to 174 fathoms. They shew farther, a decrease of 22°.50 from the surface to 40 fathoms; but from that depth to 164 fathoms, which is a depth four times as deep as the former, only 0.450. We may, therefore, imagine a stratum of water below 40 fathoms, acting as an absolute non-conductor of heat from above. From De la Beche's observations, this stratum may have a thickness of 30 to 84 fathoms. It is consequently difficult to conceive how the temperature at the bottom of the lake should ever exceed 38°.7, especially since at least twice in each year the surface of the lake falls to 38°.7, at which seasons the stratum of water bearing that temperature increases in thickness. If, notwithstanding, we find the temperature at the bottom of the *Swiss Lakes*, at all seasons, 1°.35 to 5°.74 higher than 38°.7, the water must have obtained that excess of temperature from another source, and there is no other source of heat left but the earth.

In opposition to these conclusions it may be remarked, that the water, when cooled at the surface by the air, does not sink with its reduced temperature quite to the bottom, but becomes somewhat warmed again during its descent, at the expense of the warmer layers of water, through which it passes. And this is undoubtedly the case. But since the sinking of the cold currents of water must necessarily be attended with the rising of the warmer water from below, which in its turn is also cooled when it comes to the surface; it follows, that during the continued cold of winter, the water at the bottom must at one time be reduced to a temperature of 38°.75. And if this should even only take place once during a severe winter, there being no communication of heat from above, it seems difficult to account for a temperature of 1°.35 to 5°.67 higher than 38°.75, otherwise than by supposing it to be due to heat supplied from the earth. Muncke* ascribes this higher temperature to the action of the sun, as De Saussure's observations were not made till May. But can it be supposed that the rays of the sun penetrate to such a considerable depth as that in which the observations were made, and are still capable of having any effect, when we consider how very rapidly the transparency of water decreases with the depth? It is true that Saussure's observations in the *Lake of Geneva*, in the months of February and August, show an increase of + 1°.26; but this difference might perhaps be overlooked, when we consider the great difference of the depths.

If the water at the bottom of lakes should become heated by the earth beneath, there will be a stream constantly flowing upwards, which will rise higher the less the temperature at the surface exceeds 38.°7. This stream will reach to that height in which the temperature of water is equal to that of the stream. This stream will continue to rise only so long as the temperature at the surface does not vary. But if a depression take place, there will also be a descending current which will fall to that depth in which the temperature of the water is equal to its own. If the temperature of the lake from its surface to a great depth, be thus reduced to 38°.7, the descending current will cease. In this state it will continue whether the temperature decrease towards 32°, or to whatever degree it rise. The descending current will not again come into action until the temperature at the surface, having risen above 38°.7 shall again begin to decrease. Whilst the temperature at the bottom of the lake is 38°.7, or near 38°.7, and at the surface 32°, the water having a temperature between 32° and 38°.7 may be heated by the currents made to ascend by the warmth of the earth, as high as 46°.79, without those currents reaching the surface; for at 46°.79 water has the same specific gravity as at 32°.

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* Gehlers neues physikal, Worserbuch. viii. 742. in note.

Observations on the temperature of lakes at different depths during the time when the temperature at the surface remains constant at 32° would give interesting results. For, according to the above reflections, the temperature ought to increase from the bottom up to a certain height, and then continue decreasing up to the surface. But nowhere could the temperature rise above 46°.79, so long as the water at the surface remained at 32°.* The only instance of an observation being made at a great depth, at a time when the temperature at the surface was near 32°, is that of Saussure of the 6th of February 1777, when he found the temperature at the surface 41°.74, whilst at a depth of 950 feet it was 42°.09.

Lakes receive a greater quantity of heat from the earth, the hotter the climate is in which they are situated, and the deeper they are. This quantity of heat, therefore, depends on the latitude of the lakes, their elevation above the surface of the sea, and their depth. The actual temperature of the bottom of a lake cannot, it is true, be higher than that of the water immediately in contact with it, and consequently, not much above 38°.75; but more heat will be communicated from the interior of the earth, the warmer the strata are which lie immediately beneath the bottom of the lake. Near the *Lake of Geneva*, a temperature of 62°.8 was found in a sounding well at the depth of 680 feet. The same temperature would of course be found at the same depth on the bottom of the lake, if it were entirely filled up with the solid masses which compose our globe. But 680 feet is not the greatest depth of the lake, for it measures as much as 950 feet in some parts. Supposing the increase of temperature to proceed in the same proportion as was observed in the neighbouring sounding, a temperature of 68°.34 would be found to prevail at the depth of 950 feet. These thermometrical relations, which must be imagined quite independently of the lake, were doubtless interrupted at the moment of the formation of the lake, but yet not entirely destroyed.* We may therefore still be allowed to assume 68°.34 for the temperature of the bottom of the lake, at a depth of 950 feet, although its actual temperature must be the same as that of the water at the same depth, which, according to Saussure, is 47°.72. We have therefore to distinguish between the actual temperature of the bottom of the lake, and that which it would have, considering only its latitude, its elevation above the sea, and its depth. The difference between them is the thermometrical expression, or as it were a measure, for the heat communicated from the earth to the water, and with it carried to the surface. Thus this thermometrical expression would be for the *Lake of Geneva*, at a depth of 950 feet, 68°.34 — 9°.72=58°.62. The positive quantity of heat which escapes from the bottom of the lake into the water, however, depends on the degree of conductibility of the earthy and rocky strata which lie beneath it.

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* I may perhaps have an opportunity of making such observations in the *Laacher See*, where a depth of 200 feet has been found, without there being any reason for supposing that to be the deepest spot.

In lakes, therefore, relations exist similar to those in the glaciers, namely, that there rests upon a certain surface a mass, in the one case of snow and ice, in the other of water, which continually exerts a cooling influence upon it, and reduces its temperature below that which it would otherwise have from its latitude and elevation above the sea. But since this surface is connected with strata of a higher temperature by means of a conducting medium, heat must, according to the conducting power of those strata, be continually communicated to the cooling mass. In the case of the glaciers this heat is employed in melting the snow and ice, but in lakes it is carried to the surface by the ascending particles of water. Here it either assists in promoting the evaporation of the water, and thus becomes latent, as in the glaciers, or it raises the temperature at the surface. Whether both these take place at the same time, which it is most probable they do, might be ascertained by corresponding observations on the temperature of the surface of a lake and of the earth in its immediate vicinity. Such observations, which it would be necessary to continue for a whole year, in order to obtain the mean, would therefore decide a very interesting question.*

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* With respect to this, I cannot agree with my friend Professor Muncke, who considers that the source of heat at the bottom of the lakes must already have been exhausted by time. See Gehler's n. Worterb. as above. Such a supposition could only be admitted, if we could consider the bottoms of lakes to a considerable depth as non-conductors of heat. But for such a supposition there are no grounds. To whatever depth, then, the cooling influence of the water is felt in the earth, from the same depth will the warming influence of the interior of the earth be able to exert itself; that is to say, the water at the bottom of the lakes will receive heat from the interior of the earth.

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* The annual meteorological report of *Wurtemberg* for 1831, contains the thermometrical observations in the *Lake of Constance* and the surrounding atmosphere, made by Dr Dihlmann, in July 1831, daily at two o'clock P. M. The mean of the former is 77°.25, that of the latter 79°.02. As the observations were made at the time of the daily maximum, the true mean temperature of the *Lake of Constance* must be lower than 77°25. According to observations which I made during a journey by steam to *Strasburg*, on the 19th and 20th of August 1835, on the temperature of the *Rhine*, the greatest daily difference was 1°.8 to 2°25. The true mean temperature of the *Lake of Constance* in July 1831, may therefore be estimated at about 76°.12. But from the mean temperature of the air a far greater deduction must be made. At *Stuttgard*, whose climate must be very nearly the same as that of the neighbourhood of the *Lake of Constance*, the mean temperature in July 1831 was 67°.32. According to this the mean temperature of the *Lake of Constance* must in that month have surpassed that of the surrounding atmosphere by about 9°00. In the colder seasons this difference is probably not less considerable, as the water which is cooled by the air at the surface continually sinks, whilst the ascending currents must take place to nearly the same extent in winter as in summer. From this it may therefore be concluded, that the mean temperature of the surface of the *Lake of Constance* is higher than that of the soil in its vicinity.

Ten observations on the temperature of the *Lake of Constance*, made by Dihlmann in June, July, and August 1828 (from the 13th June to the 20th August), in the afternoon and evening, gave a temperature 5°.85 higher than the mean temperature of the air on those days, and only 2°.47 lower than the temperature of the air in the shade, at two o'clock in the afternoon. Similar

So long as the temperature of the bottom of a lake be above 38°.75, currents of water will continue to rise, but they will rise more feebly, and with a smaller increase of heat, the nearer the temperature of the bottom approaches that of the maximum density of water. If in higher latitudes, or in greater elevations, the temperature of the bottom of a lake does not exceed 38°.75, an equilibrium will take place: the bottom of the lake will neither receive heat from the water nor communicate heat to it, and the lower beds of water will never be warmer than 38°.75. If, lastly, the temperature of the bottom of the lake fall below 38°.75, the water which was cooled by the air at the surface will sink to the bottom, and there become still more cooled by coming into equi-

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differences had been observed the preceding year. See Schweigger, Seidel's Jahrb. der Chemie, &c. &c. vol. lix. p. 32. Nineteen observations on the temperature of the *Lake of Constance*, between the 23d May and 30th July 1830, taken at three o'clock P.M., gave a mean temperature which was 3°.60 lower than the mean temperature of the air during the same period. Schweigger Seidel's Jahrb. vol. lxv. p. 57. There is, therefore, no doubt that the mean temperature of the air during this period was also lower than that of the surface of the lake. During frosts the mean temperature of lakes must be higher than that of the air, as the temperature of the water cannot fall below 32°, whilst that of the air descends several degrees below 32°. During the severe cold in January and February 1830, the *Lake of Constance* was frozen over: a phenomenon of very rare occurrence. Scheigger Seidel, as above, p. 36. 5000 feet from the shore, the temperature of the air was 72°.50; 2000 feet from it, —15°.25; that of the water itself, near the ice, varied on different days from 32° to + 33°.35. After the 31st January the lake froze almost entirely over, a small circle only, opposite to the *Friedrichshafen*, where the lake has *the greatest depth*, remaining open, but covered with islands of ice. Up to the end of January the thickness of the ice near *Uttweil* was found to be:

10,000 feet distant from the shore, .	6½ inches.
15,000	5 ...
20,000	4 ...
30,000	3½ ...

If, at the commencement of the frost, the surface of the lake becomes gradually cooled down, and thus gives rise to descending currents of water, which continue until the temperature of the surface reaches 38°.75; this period will naturally happen later in the deeper parts, than where the depth is less considerable; for there is a greater quantity of water, of a temperature above 38°.75 in those parts, and so long as water above 38°.75 exists below, the descending currents will continue to take place. So that the deeper the lake is, the later will its surface be reduced to 38°.75, and consequently also to the freezing point. If the depth of the lake should be found to increase in the librium with the temperature at the bottom. If in this case the lake be cooled down to 38°.75 up to the surface, ascending currents will again take place, the temperature of which will be below 38°.75, and the upper beds will be cooled down by degrees. But if the surface of the lake be warmed to as many degrees above 38°.75 as the bottom is cooled below 38°.75, no current whatsoever will take place.

Such relations must exist in the elevated alpine lakes, for example, on the *Gemmi*, the *Grimsel*, *St Gothard*, *St Bernhard*, and so forth, where the mean temperature at the bottom of the lakes is near 32°. It would therefore be very interesting if observations were to be made on the temperature in the depths of such lakes. For this purpose the small lake near the monastery on the *Great St Bernhard*, which has a depth of 33 feet, is pecu-

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direction in which the above observations were made, this would be another argument in favour of our hypothesis. The increase of temperature observed in receding from the shore, seems to be connected with this phenomenon. For as the surface of the earth on the shore was cooled to a much greater degree than the ice, the temperature of the air near the shore must be found lower than at a greater distance from it.

It is very much to be wished that accurate observers may on future occasions keep these points in view, in order that they may, by means of observations directed immediately to this subject, place that in a clear light which at present can only be given as conjecture.

Von Humboldt (Reise, &c.—III. 131) mentions, that the temperature at the surface of the *Lake of Valencia*, during his stay in the valleys of *Aragua*, in the month of February, remained constantly between 73°.4 and 74°.75. It was from 1°.08 to 2°.34 below the mean temperature of the air, whether in consequence, as he remarks, of the evaporation, by which the water and the air are deprived of a portion of their heat, or because the changes of temperature in a large body of water do not keep equal pace with those of the air, and because small streams run into the lake, proceeding from several cold springs on the neighbouring mountains. But as this philosopher found in *Cumana* that the temperature of water, after having been exposed to the sun in vessels during seven or eight hours, was always 1°.8 to 3°.24 lower than the temperature of the air in the shade; then, supposing the depression of temperature, caused in the lake by evaporation, to be equal to this, we should have for the real temperature of the lake, independently of this depression, a degree 0°.720 to 0°.9 higher than that of the air; without taking the other causes of a depression of temperature into consideration. We are therefore justified in assuming that rising warm currents of water also exist in the *Lake of Valencia*.

liarly adapted. From the above train of reasoning it would be expected that such lakes would freeze to the bottom in winter; but, according to Bieselx,[*] the last mentioned lake, although it remains frozen during eight to nine months, is usually covered with ice of only 2 feet to 2½ feet in thickness.[†] If this assertion is true, it is indeed somewhat difficult to conceive how water, inclosed from above with ice, and from below with a ground of a temperature not surpassing the freezing point, should be able to remain liquid. Could the total want of motion in the water, after being covered with a layer of ice, be the cause of its remaining liquid?

Such observations would, on the other hand, be a means of easily ascertaining the temperature of the soil in elevated regions, if that temperature were 38°.75 or below 38°.75, for then the temperature of the water in the depths would very nearly coincide with that of the bottom of the lake. It must, however, here be taken into consideration, that, if streams issuing from glaciers of a temperature nearly equal to 38°.75 flow into the lake, this will have an influence on the temperature of the lower strata of the water. During the warm season it may easily happen that streams issuing from glaciers, if they fall into the lake at a certain distance from the glacier, should have a temperature nearly equal to that of water at its maximum of density. I have also found several glacier-streams of such a temperature in the month of August at a distance from their source. But if the temperature of the lower strata of the water be 38°.75, and the glacier-streams warmer or colder than 38°.75, they will not reach the bottom of the lake. Thus, then, it may happen that lakes, in which the temperature at the bottom is above 38°.75, and into which glacier-streams of a lower temperature are emptied, may be thereby so cooled down at their surface that the heat rising from the bottom will be compensated, and consequently that the mean temperature at the surface will not be found to exceed that of the bottom of the lake. This case cannot *of course occur* in the *Lake of Constance*, mentioned above by way of example.

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[*] Gilbert's Annalen, vol. lxiv. p. 202.

[†] The water of this lake is also so cold throughout the rest of the year that no fish can live in it. I found the temperature of the surface of the *lake of Dauben* on the *Gemmi* to be 51°13, the temperature of the air being 46°.17; and that of the small lake on the *Faulhorn* at 43°.50, when the temperature of the air was 36°.50.

When the rocks which compose the bottoms of lakes, in which the temperature at the bottom is 38°.75, are very much fissured, water of this temperature will escape from them. If such water reappear as springs at a lower level, the springs will have a temperature nearly equal to 38°.75. Thus I found the above mentioned springs (chap. iv.) on the *Spital Matte*, which probably proceed from lakes situated above, to have a temperature between 37°.6 and 40°.

If the temperature of the air over a lake never fall so low as that degree at which water possesses its maximum of density, no layers of water can possibly be met with in the depths having so low a temperature as that. For the lowest temperature of the air in the cold season is the minimum of the lowest temperature of the water in the depths. From this it follows, that in the plains of the torrid zone, or in low valleys, where the mean temperature is between 77°.90 and 80°.60, the bottoms of lakes can never be below 69°.80 to 71°.60.*

In the sea, the cooling of the water at the bottom must proceed further, for the greatest density of sea-water is far below 38°.7. Marcet† has shewn that sea-water continues to contract and to increase in weight until it freezes to a solid body; since which a series of experiments have been made by Erman jun.,‡ by which he found that salt-water of a specific gravity of 1.027 has no maximum of density, so long as it remains liquid, and even when ice is formed in it, the remaining liquid part continues to increase very considerably in density. But this property must also belong to the water of the sea, which, according to Gay Lussac,§ from an average of fifteen experiments, possesses a specific gravity of 1.0286 at a temperature of 46°.4. If, then, as Erman jun. asserts, sea-water offers no anomaly in its contraction between +8° and 25°.2, it is possible that in northern latitudes water may be met with in great depths in the sea of 25°.2.

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* Von Humboldt Reise etc. iii. 133.

† In the paper read before the Royal Society of London, alluded to above.

‡ Poggendorff's Annal. vol. xii. p. 463.

§ Annal. de Chimie et de Phys. vols. vi. and vii. 1817.

The lowest temperatures of sea-water in great depths were observed by Irvine and Horner. On the 20th of June, the former found a temperature of 27°.5 at a depth of 3900 feet, in 67° N. Lat.; and the latter observed a temperature of 28°.4 in the *Sea of Ochotzk* between 30 and 115 fathoms deep. Horner conjectures that this degree is the limit below which the temperature of the sea never falls.

These temperatures are, then, still 2°.25 or 3°.150 higher than 25°.2. From which the same consequence may be deduced which was concluded from the above observations in the *Swiss Lakes*, namely, *that the water of the sea and of lakes must receive heat from the earth beneath*. The heat which the sea receives from its bottom depends, as in lakes, on the latitude, and the depth of the water. If the depth of the sea at the equator were only 6658 feet, and supposing the temperature of the earth there to increase with the depth in the same proportion as in the sounding near the *Lake of Geneva*, the temperature of the bottom of the sea would there reach the boiling point; but as the actual temperature at the bottom of the sea at its greatest depths in low latitude, according to Lenz, is 35°.96, we have 212°—3°.96=208°.04 for the thermometrical expression of the heat which is communicated from the bottom of the sea to the water, and with it brought to the surface. The positive quantity of heat thus brought to the surface obviously depends, as is the case in lakes, on the degree of conductibility of heat of the strata beneath. As the sea in the torrid zone is very commonly far deeper than the above assumed depth of 6658 feet, there must be many places in those regions where the temperature corresponding to the bottom of the sea would be much higher than the boiling point. But, even in the frigid zones, where the mean temperature falls as low as, and even lower, than 32°, much heat may still rise from the bottom of the sea even in moderate depths. The rising of heat from the bottom of the sea may, therefore, be considered as an universal phenomenon, even under the poles, and it will only be found not to take place where the sea in the frigid zones is so shallow, that there is no perceptible difference between the temperature of its bottom and of its surface.

Notwithstanding that the heat which is continually rising in the sea increases in proportion to the depth, yet no perceptible difference in the temperature at the surface can be caused by any variations in the more considerable depths, for in proportion as the quantity of heat which rises from the more considerable depths is greater, so is the quantity of water greater through which it is dispersed. But in shallows this difference must be perceptible; and this is a fact which, it is well known, was observed by Franklin and J. Williams,* and confirmed by later observers, by V. Humboldt† and J. Davy.‡ On approaching land, Williams describes the decrease of temperature as so perceptible, that coasts and shallows were indicated by the thermometer, at distances in which they were not yet visible. He not unfrequently found a decrease in the temperature of the sea of 6°.75 during a sail of three hours, and yet they were out of all danger. In some cases he even observed differences of 11°.25. The more rapidly the depth decreases on approaching a coast, the more rapid is the decrease of temperature. These great differences very clearly shew the influence of the warmer waters rising from below upon the temperature at the surface, and this influence is independent of the seasons, as the experiments also prove. If, on the other hand, Sir H. Davy's § explanation of this phenomenon were right, namely, that the sea becomes cooled at its surface by certain causes, and that in deep places the cooled layers of water are carried to a distance below the surface, whilst in shallows they remain near the surface, this decrease of temperature could only take place when the air is cooler than the surface of the sea. Neither is it to be expected, as Sir H. Davy asserts, that this decrease of temperature should not take place in very high latitudes, where the temperature of the surface of the air is near 27°.50; for in deep places, where the bottom of the sea would always possess a temperature higher than the mean temperature of the air, currents of warmed water must continually rise, and thus impart to the surface a temperature higher than it possesses over shallows. The phenomenon is, therefore, in no way dependent on the latitude, but only on the depth of the sea.

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* Transactions of the American Society, vol. iii. p. 32; and Williams Thermometrical Navigation. Philad. 1790.

† Gilbert's Annal. vol. vii. p. 342.

‡ Philos. Transac. 1817, ii. 275.

§ Journ. of Sc. vol. iii. 1817; and Gilbert's Annal. vol. lxvi. p. 130.

The higher temperature of the surface of the sea in deep places seems also to prove, that only a part of the warmed water which rises from the depths is used in the evaporation of the water at the surface. Now, as the air over the sea partakes of the heat arising from this source, but to a very limited extent, we must conclude, as in the case of lakes, that the mean temperature of the surface of the sea is higher than that of the surrounding atmosphere. And this is shewn to be the case by the observations made four times a-day (at 6 A. M., and 6 P. M., at mid-day, and at midnight) by Peron. He found the mean temperature of the water of the sea at its surface, at a distance from land, between latitudes 49° N. and 45° S., always higher than that of the air immediately in contact with it. According to Humboldt's observations, the ocean is also on an average rather warmer than the air with which it is in contact. The maximum temperature of the seas amounts to between 82°.4 and 84°.1, and the mean temperature of the atmosphere near the equator is only 78°.9 to 80°.5.[*]

The same was observed by J. Davy,[†] during his voyage to Ceylon, between latitudes 48° N. and 35° S. He made observations on the temperature of the sea and of the air with the help of two assistants, from the 12th February to the 12th August 1816, every two hours, even during the night, and from them he calculated the mean for each day. Taking the mean of all these observations, which comprises 150 days, we find, that the sea was 1°.795 warmer than the air. Of these 150 days there were only 17° on which the atmosphere gave a mean temperature superior to that of the sea, on five days they were equal, and on 128 days the mean temperature of the air was lower than that of the sea. It may, indeed, with great probability be assumed, that the observations on those seventeen and five days were made in shallower places. The greatest excess of the mean temperature of the air over that of the sea in those seventeen days was, however, only 1°.3; whilst, on the other hand, the greatest excess of the mean temperature of the sea over that of the atmosphere, in the 128 days, amounted to 7°.87. Had Dr J. Davy added the depth of the sea, which however would have been attended with many inconveniences to his numerous and valuable observations, it might have been decided with certainty, whether the excess of the mean temperature of the sea over that of the atmosphere, becomes really greater in proportion as the depth increases, and *vice versa*, whether the hypothesis that the mean temperature of the sea in shallows is lower than that of the air, has any foundation. If we take the mean of the thermometrical observations in the sea and in the atmosphere, between latitudes 5° N. and 5° S., we find for the former 79°.607, and for the latter 78°.226, making a difference here also in favour of the former, of 1°.454.

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[*] Reise, i. 353. See especially the data in the table, p. 352.

[†] Philosophical Transactions, 1817, ii. p. 275.

The above observations include the latter half of the winter and a part of the summer in the northern atmosphere, and the whole autumn with the first half of the winter in the southern hemisphere, comprising more of the cold than of the warm seasons. However, if we only take the average of the summer observations in the northern hemisphere, from the 1st to the 12th of August, we find the mean temperature for the sea 79°.953; for the air 78°.353, which even thus gives a difference of 1°.575. The higher mean temperature of the sea as compared with that of the air, seems, therefore, to be by no means confined to the cold season.

Should the water of the sea reach its maximum of density at a certain low degree of temperature, the circumstances would be similar to those above developed relative to lakes in elevated situations. Namely, in high latitudes, where the temperature of the bottom of the sea would be as low as that corresponding to the maximum of density of the sea-water, the sea would neither impart heat to, nor receive heat from, the bottom. In such places there could consequently be no ascending current of water.

If the sea was a motionless mass of water, its temperature in great depths could never fall below the minimum temperature of the air. The temperatures 46°.4 and 44°.5, observed in great depths in tropical seas, could then not be accounted for, as in such climates the temperature of the air never falls below 65°.5 to 68°. But if inferior currents flow from the poles to the equator, in the manner of the well-known stream which runs from the *Cape* towards the *coasts of Brazil* and the *Gulph of Mexico*, we must no longer be astonished at the low temperatures observed in low latitudes. *

The floating ice-bergs in the polar seas, which, according to the corroborative accounts of the whale-fishers and northern navigators, even in a calm, travel southward, seem also in certain seasons to assist in the depression of the temperature of the sea in low latitudes. †

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* On these currents see Guy-Lussac, Annal. de Chim. et de Phys., vols. vi. and vii. 1837; Lamarche, ibid. vol. v.; and Chladni, in Gilbert's Annalen, vol. lxii. p. 133; Barrow, in the Quart. Rev. Feb. 1818. No. 35; John Davy, Edin. Phil. Jour. vol. xiii., and Von Humboldt's Reise. English edition, vol. i. p. 63.

† Will. Scoresby jun., in the Wernerian Society, 1815, and Barrow, Quart. Rev. as above. In *Davis's Straits* where they are exceedingly numerous, they always travel towards the south, extending without the straits to an almost incredible distance, and are found in great numbers on the shallows about *Newfoundland*, and have been met with in lat. 40° more than 2100 nautical miles from the place of their origin. The whale-fishers are not unfrequently enclosed by them and carried away to S. W. Scoresby relates that ships have often been driven from the usual places for whale-fishing in 78° and 80° N. Lat. and a few degrees of E. Long. in a south-westerly direction into lat. 62°, and longitude 30° W. of *Greenwich*, and have still continued to be borne along. In the Autumn of 1815, the Greenland traders returning from the whale-fishery, brought news that the immeasurable plains of ice which had been amassing for the last 400 years on the coast of *Greenland*, were beginning to break up, and to drift towards the south; in the succeeding years this report was confirmed by several vessels, which, on their return to *England* from the *West Indies*, *North America*, and *Newfoundland*, had met with great fields of ice and lofty icebergs, floating southwards. From latitude 46°.5 to 40°, they continued to be encountered by vessels; indeed, in the summer of 1818, they even proceeded as far as *Cuba* (22° N. Lat.). In the southern hemisphere, floating ice mountains are also met with in extraordinarily low latitudes—in latitude 44° and even 36°. See Phil. Trans. for 1830, Part I. p. 117.

But whether this phenomenon is so extensive as to cause, by the melting of such masses of ice, not only a local but a general cooling of the sea in low latitudes, is difficult to determine. 'Tis true navigators have met with masses of ice of several nautical miles in circumference; indeed, one has been seen eight miles in width, and the whole length of which could not be compassed by the eye, and another of such dimensions that a ship required three days to get clear of it, whilst at the same time it rose to a height of 100, 200, and 250 feet above the sea, and must, therefore, have extended to the depth of 450, 900, and 1100 feet below the surface. But, although considerable, what are these masses of ice compared with the enormous quantities of water of the ocean in the temperate and torrid zones? Yet, since several natural philosophers have sought the cause of the cold damp summer of 1816, and a part of 1817, which was so prejudicial and destructive to a great part of *Europe*, in the melting of those enormous masses of ice,* and since, in *Great Britain*, the thermometer was observed to fall whenever the wind blew from the west, it may well be supposed that those masses of ice do actually exert some influence on the depression of temperature in the sea in low latitudes.

As, notwithstanding these causes of a depression of the temperature of the tropical seas, their temperature is still higher than the mean temperature of the air, with which they are in contact; and, as farther, the water of the sea loses some of its heat by evaporation—other causes must cause an elevation of their temperature, which overbalance all these. The water of the sea does indeed receive more heat from the rays of the sun than the air; but it also, on the other hand, loses heat in the night by radiation. It is true we are not able to measure the relative powers of the warming and cooling influences which operate on the surface of the sea; but it is hardly to be supposed that the former could be the most powerful, unless the sea received heat from the earth at its bottom.

To be continued in next Number.

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* Chladni, in Gilbert's Annal. vol. lxii. p. 132.

On the principal Geological Phenomena of the Caucasus and the Crimea. In a Letter from M. F. du Bois, to M. Elie de Beaumont.

M. Du Bois has discovered several epochs of (soulevement) elevation in the region of the Caucasus. The oldest of these which he has clearly recognised, is posterior to the Jura formation, which of course has participated in the convulsion. Granitic masses, he remarks, have pierced the thick mass of black schist, and have dislocated it, in heaving aside the strata of Jura limestone, which were superimposed, and thus bursting the crust of the earth have raised from the depths of the ocean the first rudiments of what might be called the island of the Caucasus, and which was elevated several thousand feet above the level of the surrounding sea. An epoch of repose succeeded to this first cataclysm, and sedimentary deposits followed the first uprising. During this period of repose, the lower schist of the chalk and the green-sand were calmly deposited. Each of these stages of deposit produced a formation of many thousand feet in thickness. The conclusion of the epoch of the green-sand, appears to have been marked by a new upraising, viz. that of the Akaltsikhé chain, the axis of which approximates in its direction to that between east and west, very nearly corresponding to that of the elevation of the sandstone and the marl in the Carpathian chain.

The principal agent, in this new revolution, was melaphyre, or pyroxenous porphyry; it has cleft the chain in the greater part of its length, and has broken forth by this gap, turning aside on either side the two slopes of the chalk, under an angle of 30°, more or less, not at all unlike the roof of a house. This circumstance may very readily be observed in the journey from Koutaïs to Akaltsikhé, across this range, which is nearly 10,000 feet high.

After this second upraising, there existed, according to M. du Bois, between the Caucasian island already referred to, and the Akaltsikhé range, a frith or sea, into which was deposited true chalk, along with all its characteristic fossils, and to which there still corresponds that long depression, under the name of Colchis and Georgia, which skirts the southern acclivity of the Caucasus. To the south of this depression, the traveller, continues M. du Bois, finds himself in a labyrinth of volcanic amphitheatres, analogous to those which are seen upon the surface of the moon, and which here, crowding upon each other, fill the whole space between the Caspian and the Black Sea. In traversing the peaks of Ketedagh and Kiskala, the volcanic amphitheatre of the Lake Sevang is seen, elevated 5000 above the level of the ocean. It is quite surrounded with volcanos, and by jets of various trap-rocks and porphyries, which yield a small streamlet during the spring months, but which dries up during the rest of the year. This water is fresh, as is that also of the lake, whose dimensions, according to the late Russian trigonometrical survey, is 15 leagues long (French), 8 broad, with an area of 78 square leagues.

To the north-west of this volcanic amphitheatre lies another, viz. that of Somkhétij, where are found those beds of lava and obsidian, which have their source in the mountains of Trialethi, and which have almost surrounded the Kram and the Alghet.

To the south-west of the Lake Sevang, as if to proclaim in express terms what now is, and what formerly was, you pass from a vast amphitheatre filled with an immense mass of water, to the vast central amphitheatre of Armenia, now emptied of its fluid contents. The Kiotangdagh, the Agmangan, the Naltapa, and many other craters and volcanic cones, separate these two amphitheatres; whilst the great Ararat, with an absolute height of 16,254 feet (French), the small Ararat, whose height is 12,162, the Sinak, and the Takhaltou to the south, and the Alaghez, 12,000 feet high, to the north-west, finish with their imposing cones the rest of that superb garland of extinct volcanos, whose agency has assisted in filling the basin of central Armenia or Ararad. Over all its circumference, nothing is to be seen but black and gray lava currents, with pumice or obsidian, along with scoriæ and basalt or trass, intermixed with porphyries and melaphyres.

In passing from the banks of the Araxes to those of the Kour, you encounter the volcanic amphitheatre of the high Kour or

Akaltsikhé. Over a vast space, of which Kertris perhaps is the centre, you find nothing but pyroxenic lava, along with cones of ashes, beds of scoriæ and lapillæ.

I have myself, says M. du Bois, visited the whole of these amphitheatres, which, taken together, are the true key by which to explain those other enigmatical amphitheatres we find filled with fragments of quondam seas, or small Mediterraneans, more or less salt, known under the names of lake Van and Ourmiah, and which have no rivers of egress. The larger of these, the lake Ourmiah, is 27½ leagues long, by 8½ broad, and its surface measures 200 square leagues; the other, lake Van, is 22½ leagues long, 15 broad, with a surface of 176 square leagues.

All the volcanic phenomena last described are more recent than the elevation of the Akaltsikhé range, or that of the greensand.

But all these fragments of basins, these volcanic uprisings, are nothing more than isolated and partial effects, which were more or less independent of each other. They were but feeble preludes to the last great effect, which was by far the most extensive; for it has certainly upraised the Caucasus to a much greater height than it previously possessed, and elevated it to the height it now actually reaches, and it was the means of laying bare and dry all those arms of the sea, so to speak, which surrounded it; in other words, the regions of Colchis, Georgia, Daghestan, and all the vast steppes which so extensively surround the Black Sea and the Sea of Azof, and which cover the Crimea.

Not only did it form a volcanic outburst on the south of the Caucasus, but it gave birth to many volcanic chimneys in the very heart of the chain; the chief of these vents of eruption are the Elbrous, the Passeinta, Kasbek, the Red mountains, &c.

In considering the whole district around the Elbrous, we cannot hesitate therein to recognise the results of an immense crater of eruption and elevation. Trachitic porphyries break forth across black schists, and perhaps likewise through the granites and diorites, which we found at the foot of the principal cone. The schists have been raised equally high, and have been overlaid. The Jura system and the analogous formations, surround with their great solid masses the hollow of the crater of elevation; and the chalky schists, the green-sand, the white chalk, and the whole lower ranges above the Jura system, present all their strata ascending towards the central cone, with an inclination always increasing the nearer they approximate to the foot of the cone.

But that which best of all, by its lava, characterizes the volcano of eruption, forms that portion of the Red Mountains which overlooks the village of Kachaour, upon the great road between Tiflis and Wladikavkas. Two or three cones are raised up against an enormous wall of black schist, which is elevated 9000 or 10,000 feet, and whose strata have been so arranged that they present their outgoing to the cones, over which they predominate. Currents of lava have filled up, to a great extent, the large crevice, or more properly, the valley at the bottom of which the Aragri flows.

During the whole of the tertiary epoch, injections of melapyre, and other pyroxenous porphyries, have never ceased to occur in the depression of Colchis and of Georgia, between the volcanos of the Caucasus, and those of Armenia. A great number may readily be observed over the whole of the ancient Colchis, in Karhtilinia, &c.; some of these belong to the era of the upper chalk, but the greater number are as late as the tertiary epoch. They explain how the different formations of the tertiary system may be found at heights so greatly different, and in circumstances, as it regards position and derangement, which is only to be explained by their concurrence. We may now discover strata of the upper tertiary, at Bagdad upon the slopes of the Akaltsikhé mountains towards Colchis at a height of 1500 or 2000 feet; and upon the opposite slopes of the Caucasus at Tchekoühi, in the valley of Phase, a somewhat older formation is elevated as much as 3000 and 3500 feet. Between Jor and Alazan in Georgia, the upper formations attain a height of more than 2000. A hundred examples of this sort might easily be cited.

Such is a summary, concludes M. du Bois, of the history of the land which the last revolution of the globe elevated, and laid bare, free from water, as it now exists. A question here occurs, have the volcanic phenomena ceased now? and in answer he replies that he decidedly doubts it. But, be that as it may, the frequency and the violence of the dreadful earthquakes in Armenia are ever and anon proclaiming the real character of the foundation on which it rests.

On the Occurrence of Glanders in the Human Subject. By Dr W. Eck.*

THAT the introduction of contagious morbid products, derived from the lower animals, into the human system, is capable of producing much good and much evil, is universally known. The protective of vaccination, and the disastrous effects of inoculation with the hydrophobic virus, affords striking examples. The experience of modern times has taught us, that many contagious diseases of the lower animals exercise a pernicious influence on man; of these, I may mention mortification of the spleen, and its consequences, the black pock *(Schwarze Blatter)*, and glanders, with its effects on man.

The phenomena of glanders (a disease attributable to lymphalic cachery by Veith, and ranged among the tubercular affections as a specific degeneration of the cellular tissue by Dupuy), in its acute or chronic form, are well known, and have been graphically described in the official report on contagious diseases. We are also well acquainted with the phenomenon of button farcy, a disease which bears a close relationship to glanders, frequently conjoined with it, and generally arising from one and the same source, occurring only in the genus Equus, and its varieties, and distinguished chiefly from glanders by the affection of the cutaneous lymphatics, while, in the latter disease, those of the internal surface are engaged. It is likewise generally admitted, that glanders may become spontaneously developed in the horse, by errors in the nutritive functions, that is to say, under conditions which gives rise to the morbid state of the lymphatic system, and particularly of those glands which have a more intimate organic connection with the mucous membrane of the nostrils. Authors, however, are less generally agreed as to the power of the infection which this disease possesses with respect to animals of the Equine species. Even in the most modern times, it has been expressly denied by Godine, Dupuy, and others. The inconvenience of many police regulations, founded on a conviction of the contagious nature of the disease, has favoured the partial introduction of the opposite opinion, which the uncertainty of the diagnosis in the first stage, and the fact, that a horse labouring under glanders, continues for such a length of time in the apparent enjoyment of general good health, has tended to corroborate. Again, where one horse labours under glanders, and a second or a third becomes affected, it is maintained that each have been exposed to the influence of similar atmospheric or local causes; and where a horse exposed among a team of glandered horses, remains free from the disease, it is looked upon as a convincing proof of its non-contagious nature. On the other hand, the inoculations made by Viborg,* and his extensive experience of the destructive consequences of the foregoing views, have diffused more widely the conviction of the contagious nature of glanders. Besides, the reports of almost every provincial board of health, in modern times, have furnished cases of the kind; and all our medical police counsels have assumed as *proven* the contagious nature of glanders, a fact of which no experienced dealer in horses now entertains a doubt. And, though the fluid which flows from the nostrils is recognised as the chief source of infection, and that danger is chiefly to be apprehended from the use of utensils, mangers, and drinking vessels, &c. which have been in contact with the glandered horses, and which are so likely to come in contact with the mucous membrane of the nostrils of sound horses; still we can scarcely doubt that the contagion may not also exist in other excretions; and that occasionally, as for instance, in close damp stalls, sound horses may become affected without any contact, probably through the medium of the inspired air. That many horses are less susceptible of this disease; that the intensity of the disease itself differs in degree; that the matter of glanders loses its infectious properties by exposure to heat and fresh air; and the sound horses may remain for months in company with glandered without taking the disease, provided a certain species of communication be prevented; these are circumstances which are analogous to those with the other species of contagion, and do not militate against the supposition of their infectious powers. With regard to infection, glanders and button farcy preserve an identity in the dyskrasy to which they owe their origin in this point also,—that the latter is produced not alone by inoculation with the purulent matter generated in the ulcers, but by inserting the matter which flows from the nostrils of a glandered horse into the skin of a sound one; and *vice versa*, glanders is produced by inoculating the mucous membrane of the nostrils with matter taken from an ulcerated spot of button farcy.

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* Dublin Medical Journal, vol. xii. p. 73. Translated from the Medicinnische Zeitung for May 1837.

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* Uber Potz, Purm und Kropfder Pferde, in Seinen Sammlungen. Band. 2 and 3.

The injurious and highly pernicious effects of glanders on the human subject is much less generally admitted. Up to a very recent period, only a few veterinary surgeons recognised the fact of its destructive properties in their published treatises, and even then, for the most part, with certain reservations. Thus, for instance, Professor Waldinger* says, "In dissecting horses who have died of glanders or button farcy, should the operator cut himself, great care must be taken not to let any pus get into the wound, as the most deplorable consequences, and even death itself, are to be apprehended." From this it appears that the only dangers he dreads, are connected with the opening of bodies, and from pus getting into a wound. Director Veith† observes, "The specific effects of the contagion of glanders act solely on animals of the horse species, and operate on other domestic animals, chiefly as an acrimonious animal fluid. In man, inoculation with the matter of the glanders (which generally occurs from a cut finger coming in contact with the matter, or from the matter getting into the eye when the animal suddenly and forcibly expels it from the nostrils), brings on violent inflammation of the parts, which is extremely painful and obstinate, involves by sympathy the neighbouring lymphatic glands; and bears a resemblance to the arthritic inflammations." From this it would seem that Professor Veith was merely acquainted with the injurious effect of the poison on man. The late director of the veterinary school in this city, Professor Naumann, and Veterinary surgeon Halbach,* when asked their opinion as to the infectious power of glanders with respect to the human subject, in a suspicious case, which was under the care of Dr Schilling in 1821, stated, "That they had not met with any example of this disease in man, from glandered matter received from a living horse, and that none of the veterinary surgeons or grooms who had been occupied with living or dead glandered horses, had ever been infected. On the other hand, however, there were not wanting instances in which persons who had cut themselves, while making preparations from such bodies, occasionally had bad forms of inflammation, and even mortification affecting the hands and fore-arms." About the same time also, veterinary surgeon Major Dietrichs,† disputed the possibility of the human subject taking infection from a living horse, but admitted that he had met with some cases in which men who cut themselves while dissecting the bodies of horses who died of marasmus and glanders, became ill, and mortification of the injured part, which proved fatal. Nay, Surgeon Hallbach was so little afraid of infection from living horses, that he offered to inoculate himself with the matter of glanders.

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* Warnehmungen an Pferden. 2te. Aufl. Wien, 1810. p. 95.

† Handbuch der Veterinairkunde. 2te. Aufl. 1822, S. 685.

The fact of the existence of such opinions among experienced veterinary surgeons, the few opportunities which most practitioners have of observing the disease in man, the ignorance of its symptoms among former observers, and the false representation which have been given of cases, will be sufficient to explain the prevalence of the foregoing views among the majority of medical men ; and hence, I have not been much surprised to hear not long since, from medical functionaries, and practical physicians of the first class, similar opinions as to the unproved, or at least hypothetic and improbable noxious qualities of glanders with respect to man. Since 1821, however, a different opinion has prevailed, founded on the experience of hospital, military, provincial, and veterinary surgeons, and has been communicated to the public in official reports, dissertations, and widely-circulated journals. Among these I may mention the interesting case of an artillery-man named Rennspiess, detailed by Dr Schilling in November 1821.[*] In this case there was no direct certainty of infection from glanders; but it was extremely probable, as the man had been attending and grooming some glandered horses, and stated that he had frequently washed the ulcerated nostrils of these animals.

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* Rust's Magazin, B. xi. S. 500.

† Ib. B. xi. S. 510.

In the same month, the case of a groom named Kliech occurred, and was communicated to the Medical Council of Silesia by Dr Weiss, the district-surgeon of Neumarkt.[†] In 1822, an interesting description was given by Tarozzi, of a pestilential disease which appeared about the beginning of 1815 among several men in a stable at Ostiano, where there were glandered horses, and at a time when there was no trace of any such disease in the vicinity.[‡] To these may be added the experience of Schrader,[§] Travers,[||] Numann,[¶] Elliotson[**] and Williams;[††] Brera's Treatise on Carbuncular Typhus produced by Glanders;[‡‡] the series of accurately detailed cases given by Professor Hertwig,[§§] and my colleague Worlff;[||||] the inaugural dissertations published on this subject at Berlin by Grub,[¶¶] Krieg,[***] and Barth;[†††] as also two remarkable cases communicated by Professor Alexander, Director of the Hospital of Instruction at Utrecht, and accompanied by an accurate detail of the post mortem phenomena. I may also state, that my friend Berndt has devoted a particular section to this subject in his admirable work on Pathology and Therapeutics.

After adducing so many instances drawn from the stores of modern observation, and confirmed by practitioners of various countries, the attempt to render conviction stronger may appear superfluous; but when, even in 1832, we find Parent Du Chatelet, in a report made to the Academy at Paris, maintain that "none of the supposed infectious diseases of animals exercised an unfavourable influence on the health of man;" when further, in a widely perused journal published in this city,* we find a practical physician, Dr Krüger Hansen, in examining Wolff's proposition above mentioned, denying the existence of infection from glanders in general, and its pernicious effects on man in particular, and in pretty plain terms attributing all the bad consequences which arises from such infection to inaccurate diagnosis and bad practice; finally, when we perpend the truth and importance of Göethe's words, "that the truth must be constantly repeated, because error is preached upon every side, not only by individuals, but also by the mass of mankind," it becomes our duty not to hold back any communication, however trifling, calculated to banish a pernicious confidence, and to contribute to the knowledge and treatment of an animal poison, which, though seldom observed, still, when it once occurs, whether openly or in a latent form, threatens the most alarming consequences to human life. These considerations have induced me to publish the preceding observations.

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[*] Rust's Magazin, B. xi. S. 480.

[†] Rust's Magazin, B. xi. S. 480. Hufeland's Journal for March 1822.

[‡] Omodei's Annals for 1822.

[§] Hamburg Magazin for Jan. and Feb. 1823.

[||] Inquiry into Constitutional Irritation.

[¶] Vee-Artsenykund Magazin, Deel. ii. St. 2.

[**] Medico-Chirurg. Trans. vol. xvi. p. 171.

[††] Medical and Surgical Journal, No. lvii. p. 156.

[‡‡] Anthologia Medica for Sept. and Oct. 1834.

[§§] Medicinishe Zeitung, Jahrg. 3, Nos. 46 and 47.

[||||] Med. Zeitung, Jahrg. 4, Nos. 1 and 2.

[¶¶] Diss. Sistens casum singularem Morbi Contagio mallei humidi in hominem translato orti, 1829.

[***] De Typho Malioide, 1829.

[†††] De Nonnullis Epidemiis et Epizootiis simul regnantibus earumque mutua Indole Contagiosa, 1835.

Dr Eck gives an account of severe cases of disease consequent on actual or supposed infection from glanders; and in Volume 5th of the Transactions of the Provincial Medical and Surgical Association, there is a case of glanders in the human subject.†

SCIENTIFIC INTELLIGENCE.

ZOOLOGY.

1. *The European Bison; Bos urus;* the *Bœuf Auroch* of the French.—At a meeting of the Imperial Academy of Sciences at Petersburg in autumn last, M. Baer read some observations upon the above named animal, which were suggested by the reception of a skin, which had been sent to the

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* Gräfe and Walther's Journal, band. 23, heft 1, s. 58.

† *Vide* Dublin Medical Journal, vol. xii. for Dr Eck's cases.

Academy by General Rosen from the Caucasus. This animal, which is known by the name of *Aurochs* in France and Germany, and by that of *Zoubre* in Russia, and which Cuvier has shewn to be the same as the *Bison* of the ancients, the *Wisent* in Germany, was, in remote periods, spread over nearly the whole of Europe. Many names of places, as *Wisantensteg* and others, are a memorial of it in Swabia. Its chase is sung in the Nibelungenlied. At the time, however, of the revival of letters it was no longer known in Germany. It maintained itself for a longer time in Prussia, and in different parts of Poland, where it was seen and drawn by Herberstain. The last that was killed in Prussia was in the year 1755. The younger Forster tells us that in his time it was found in Poland only, in the great forest of Bialowicza,* from which it would have been extirpated by this time but for the care with which the Russian Government watches over its preservation. This for a considerable time has been regarded as the only locality where the Bison was to be found. It is therefore an interesting piece of intelligence to the student of zoology to be informed of its presence in the Caucasus, where are still to be met with the royal tiger and the panther. M. Baer has instituted a minute comparison between the portions lately transmitted from the Caucasus, and the specimen already in possession of the Academy, and which had been brought from the above named forest of Bialowicza, and has found that in the former the horns are sensibly more slender and shorter, and that the distance which separates them or the breadth of the forehead is less. At the same time he conceives that these differences proceed only from the difference in sex, the Caucasian individual being a female. The colour of the hair is moreover not so deep, and is mixed with grey; it is also shorter in all the anterior portions of the body, and is curled only on the forehead, and on a part of the neck; but M. Baer still explains these differences as arising from age, the season of the year, &c. The hoofs and the ergats —those short horny stubs placed behind and below the posterns —are much shorter than in the Polish animal, which is probably connected with its mountainous habitat. There are no other differences between these two bisons, so far at least as can be judged from their skins, except a slight difference in the curve of the horns, and the presence of a dark coloured streak which runs along the back of the one, and is not present in the other. These differences, it will be perceived, are quite insufficient to enable us to conclude whether the wild bull of the Caucasus is to be regarded a distinct species from that of Lithuania, and it is only by the examination of the skeleton that this can be determined. It isnow several years since notice was given of a wild bull named *The Gaour*, *B. Gaurus*, in the interior of India, between the coast of Coromandel and the Bay of Calcutta. The existence of a zoubre or bison in the Caucasus has led M. Baer to infer that this bull is also a bison, the incomplete description which has been published corresponding with sufficient accuracy with what is known of the Caucasian animal. M. Baer also conceives it very probable that the same animal is found on the other side of the Ganges. He grounds this supposition upon the recital of Captain Low, in the Journal of the Asiatic Society of London. Finally, and moreover, he does not doubt that it now sojourns in the central parts of Asia, and extends even towards its eastern portion. In fact he agrees with Schmidt in thinking that the Mongolian writings allude to this animal, where they mention a wild bull which frequents the environs of the lake Kokkonoor, and also the Chinese province of Khansi; which is distinguished from the *Yak*, *Bos Grunniens*, and which the Mongolian dictionaries thus describe. "It resembles a common ox; the anterior portion of its body is high, the posterior sloping and narrow; its coat is of a deep-slate colour, or deep brown or blackish." The zoubre or bison, then, he remarks in concluding, is still at the present time dispersed into several herds and tribes, widely separated from each other. In the forest of Bialowicza it has for its companion the wolverene, Ursus Gulo Lin., and on the coast of Tenasserim the elephant and the rhinoceros. If now we recur to the notion of Pallas, who, struck with the similarity of the bison of America and the aurochs of Europe, and considering and imagining that this latter animal was not to be found in Asia, affirmed that the European animal had travelled from the West, we shall be led to conclude that there are good grounds for questioning this opinion. As bearing on hese changes of the habitat of this urus, M. Baer makes some eflections upon the variations which the geographic distribution of animals undergo, which may be inserted here. Some animals, he remarks, travel with a particular vegetation, and others along with man; some have been presented to Europe from America, and others have passed over from the old world to the new. Amongst the Mammalia it is always the smallest, belonging to the Glires, and the Insectivora, which most prevail. The very smallest of the mammalia, the pigmy shrew-mouse, *Sorex pigmæus* of Pallas, which was never before seen in Germany, has within these few years been discovered in Silesia and in Mecklenburg. Many species of mice and rats are continually advancing from Asia into Europe. At the present time the black-rat (Mus Rattus) is no longer the common rat, but another stranger species, so new that Linnæus was not acquainted with it, and the epoch of whose arrival at Astrakan is ascribed by Pallas to the year 1727, has effected the disappearance of the former wherever commerce is established. This visiter is the *Surmulot* of Buffon, the *Wanderratte* of the Germans, the *Mus decumanus* of Pallas. It has been transported in our day by the Nadejda to Kamtschatka; in fact, it might be adopted as the appropriate ensign of commerce, and we might safely say that a place without the brown rat is a place destitute of commerce. On the contrary, the larger animals always retire, and finally become extinct, a proof that the issue of the contest between man, and any animal, whatever be his strength and courage, is always in favour of the lord of creation. It is thus that the lion, which, according to Herodotus and Aristotle, still existed in their times in Macedonia, after having for a long time occupied Asia-Minor and Syria, is now-a-days repelled from the frontiers of Persia and India, into some desert portions of Arabia, and is dominant solely in Africa. So, too, the crocodile no longer exists in Lower Egypt; and the hippopotamus and cameleopard, and other colossal animals, have retreated into the interior of Africa. But there are likewise species of animals which have been completely extirpated even within the period of historic records. Thus the urus of the ancients, which, in the time of Cæsar, was common in Germany, no longer existed there in the sixteenth century. And the sea-cow of the Kamtschatka seas has a still shorter history. In fact it was only towards the commencement of the eighteenth century that it was observed, and known. Steller gave a detailed description in the year 1743 and in the year 1768, that is to say, in twenty-five years, after the last individual appears to have been destroyed.

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* An interesting account of the Forest of Bialowicza will be found in one of the volumes of this Journal.

PHYSIOLOGY.

2. *Upon the Affinity which the Fluids of living organized Structures have for Water.* By M. DE BLAINVILLE.—Naturalists have long observed how much the hygrometrical and statical condition of the atmosphere, in other words, how much its moisture and weight, exerts an influence over the life of animals, influencing their form, whether small, sleek, and elegant, or on the contrary, heavy, lumpish, and swollen, according as this medium of existence is dry and in brisk movement, or on the reverse, saturated with humidity and stagnant, as is well exemplified by comparing together the inhabitants of Holland and Andalusia. They have also noticed that the habitual use of free potations and watery viands, or on the contrary, very nourishing and dry food, have a manifest effect on the condition of man and animals. As, however, these different results require a longer or shorter time before they take effect, their mode of action, we conceive, cannot be so manifest as it was on the occasion of an observation made in Egypt by M. P. E. Botta, a naturalist connected with the Natural History Museum of Paris, and now travelling in Arabia Felix. His remark is this: Camels, as is well known, are the only means of transport which can be employed in traversing the vast deserts which are encountered in many parts of Africa and Arabia, and this on account of their nature, and still more their habits in which they are reared from their youth, in virtue of which they acquire a power of abstinence to an extraordinary extent, being enabled to refrain from eating, and especially from drinking, during almost an incredible space of time. Here, however, we may remark in passing, that this remarkable power is not to be attributed to the circumstance that these animals are provided with a sort of stomachic reservoir, in which they husband their supply of water, as has been long alleged, and is still repeated in many of our modern publications, but is owing merely to the great extent of the salivary pparatus, which in all animals has a development in the ratio f its common food. Now, during the long journeys across

the portion of the great desert which begins or ends at some distance from Cairo, according as you ascend or descend the Nile, M. Botta had occasion to observe that these camels, in proportion as they proceeded from the place of departure, became thin with a rapidity which was very striking and remarkable. He had also occasion to confirm an observation which was made ages ago, that these animals really seemed to smell water at incredible distances, which is inferred from the increased swiftness in the speed of the camels; which, in spite of their enfeebled power, during the progress of a protracted journey, redouble their efforts as they approach the spots where water is found, in hopes of obtaining that refreshing draught which can alone satisfy their thirst and relieve their torments. So soon as these animals reach one of those springs so sparingly scattered over the desert, they throw themselves into it with avidity, and though more or less muddy, they continue drinking it for a long time. That which most of all astonished M. Botta was the almost instantaneous change which this treat produced in them. In fact, after having been in this way reduced to the worst and most meagre condition, after the expiry of a short period of repose, and having drank well, they rise in so much apparent flesh and good condition, that you could scarcely believe them to be the same animals. Since, therefore, there was no other change in their regimen than the introduction into their stomach of a handful of dry nourishment, and of a great quantity of water with which they had just before gorged themselves, it is clear that this alteration in their condition, so sudden and so apparent, can be attributed only to the introduction of the watery fluid first into their stomach, then into the circulating fluids, and, finally, into the cellular tissue, in consequence of a true act of imbibition through the continuity of substance, whether circular or capillary, as in a sponge, and perhaps also by that process which is now denominated *endosmose*; that is to say, by the affinity which the liquids of a living organized part have for moisture, after they have been deprived of it by great exhalation.

ANTHROPOLOGY.

3. *On Toothach from Caries, by Troschel.*—This author has followed up some observations, made by him last year in a

Prussian Medical Journal, in which he endeavoured to prove that the violent pain which occurs in caries of the teeth is not caused by the laying bare of the nerve, and that caries, if unaccompanied by any other aliment, is in most cases free from pain. There are exceptions, however, to this rule which are not uncommon. We find ordinarily two or more carious teeth together, of which very often one gives great pain, and the others which are much more injured, and in an apparently worse condition, give no pain. Despite of all palliatives, and all possible attention in the avoidance of cold, the pain often lasts whole weeks with increasing or decreasing violence, there is congestion and repeated swelling of the face, sleep and appetite are banished, and even the good constitution of the sufferer begins to be affected. After the tooth, the author of all this suffering, has been drawn, all complaints cease, and the patient soon recovers. If the extracted tooth be now broken in two, or, what is better, sawed longitudinally through the centre, we find that from the carious part, which is often very distant from the nucleus, there extend a black or brown streak into the cavity of the tooth where the nerve lies. Sometimes this streak is not very distinctly marked, and in this part the substance of the tooth is only a little less white, duller, and more pellucid than the surrounding structure. This change of colour occurs on this account, because that the canals in the substance of the tooth, which lie in layers close one behind another, and pass from the circumference to the centre, are permeated with pus (according to the examinations of Purkinje, Valentin, Gurlt, and Muller;) they are denominated by the last mentioned author, "caniculi chalicophori." In caries of the crown of the tooth, the phosphate of lime which is contained in these canals is absorbed, and during the suppuration, the carious matter filtrates still further from the base of the abscess into these little pores: then not only the white colour is lost, but the nucleus of the tooth (the nerve of) becomes affected, and this causes the most intolerable pain. Every dentist of observation has seen those dark streaks which pass to the nerve; the little canals can, ever, only be seen under the microscope, and then only on sections of the tooth prepared on a grinding-stone. It is from very acrid applications, and such as for a period paralyze the nerve, that any alleviation is to be obtained from the torture one suffers, and which arises in the manner we have described. Even the application of the actual cautery to the carious hollow, has no lasting effects, and the extraction of the tooth remains as the only resource.—*Dublin Medical Journal*, vol. xii. p. 136.

4. *Egyptian Dancing Madness, and Fire-Eating.*—Professor Hecker has written a valuable and elaborate history of the dancing madness that seized multitudes of religious fanatics in the middle ages, and of which the name is still preserved in our nosology, under the title of *St Vitus's Dance.* The effects of the various positions and motions of the limbs and body on the mind have not yet been studied by physiologists with all the attention the subject deserves and requires. That attitudes and postures exert a very important influence on the mind, may be proved by the effects of the manipulations used by the practisers of animal magnetism, and by the testimony of actors who acknowledged that it is difficult to assume the posture indicating any passion, without feeling more or less of that particular emotion. We cannot throw ourselves into the attitude of the striking combatant, without feeling somewhat of the ardour which would give strength to his blow; neither can we imitate the shrinking posture of the terrified, or the headlong flight of the pursued, without partaking more or less of their fears. To a certain extent this circumstance, combined with the contagious nature of fear, may explain the difficulty of rallying troops if once they have turned their backs to the enemy; and even the bravest and best disciplined soldiers, in retreating leisurely before an advancing foe, find it a task to proceed in good order. The attitudes of the female dancers at Gades, described by Martial and Juvenal, and those of the Egyptian public singing girls called *Ghawazee*, exert an influence over the passions not only for the spectators but of themselves. Some dances consist of motions calculated to excite an amorous, some a martial spirit. The latter are the chief favourites of barbarous, the former of more polished nations; and without fear of giving offence, we may be permitted to rank the waltz among the physiologically erotic species of dancing, although we do not quite agree with Byron in unconditionally reprobating its introduction amongst

the English. Again, among the ancients the value of forms in encouraging feelings of devotion or respect, seems to have been fully understood, and certain postures were accordingly scrupulously enforced in the ceremonies of religious worship, or in the respects paid to kings and princes. Hence the different values attached in different parts of the world to prostrations and genuflexions, when a subject approaches his sovereign; matters which the unthinking regard as mere idle ceremonies, but which the physiologist must consider as founded on the fact, that these positions do actually increase the awe felt on such occasions. The priests and priestesses most celebrated among the ancients never thought themselves inspired, never ventured to utter oracles, even at Delphi, until they had worked themselves into a frenzy, by a quick succession of forced attitudes and grimaces. In Grand Cairo, at the public festival of the Mohhaaram, and others kept periodically, the whole population of Cairo, says Mr Lane, is on the move, when the crowding, jostling, and pushing in the narrow streets and in the mosques is quite intolerable. "At these times the convolving and dancing dervises are performing their tricks over every part of the town, blasphemously bawling out the name of God, and asking charity in the terms of the Koran." Mr Lane says, "that each seemed to be performing the antics of a madman; now moving his body up and down, the next moment turning round; then using odd gesticulations with his arms, next jumping, and sometimes screaming; in short, if a stranger observing them were not told that they were performing a religious exercise, supposed to be the involuntary effect of enthusiastic excitement, he would certainly think that these dancing dervises were merely striving to excel each other in playing the buffoon." We cannot agree with Mr Lane in this opinion, and have no doubt that the motions of the frantic dervises, properly analyzed, would be found essentially different from those of a buffoon. Thus, says the writer of an article in the Quarterly Review, they dance and whirl till they become as crazy as our own Irvingites with their gibberish bowlings in an unknown tongue; but the feat performed by one these enthusiasts is so surprising that we must transcribe it. n the middle of the ring was placed a small chafing dish of ed copper, full of red hot charcoal; from this the dervise just spoken of seized a piece of live charcoal, which he put in his mouth; then did the same with another and another, until his mouth was full, when he deliberately chewed these live coals, opening his mouth wide every moment to shew its contents, which after a few minutes he swallowed; and all this he did without evincing the slightest pain, appearing during the operation, and after it to be even more lively than ever. The other dervise before alluded to as half naked displayed a remarkably fine and vigorous form, and seemed to be in the prime of his age. After having danced not much longer than the former, his actions became so violent that one of his brethren held him; but he rèleased himself from his grasp, and rushing towards the chafing dish, took out one of the largest live coals, and put it into his mouth. He kept his mouth open for about two minutes, and during this period, each time he inhaled, the large coal appeared to be almost of a white heat; and when he exhaled numerous sparks were blown out of his mouth. After this he chewed and swallowed the coal, and then resumed his dancing."—*Ibid.* p. 161. *From Lane's Manners and Customs of the Modern Egyptians.*

5. *African Poisoning.*—The following *poisoning scene* presents some remarkable points of resemblance to similar scenes enacted by the enlightened Athenians, and the effects of the drug, and exercise, and water, in accelerating its action, will strongly remind the reader of the death of Socrates—an interesting fact, when we recollect that history points to Africa as the source of Grecian civilization. The composure of the two sufferers seems likewise to have rivalled that of the celebrated sage of antiquity, although based on less philosophical grounds—this too may form a subject worthy of deep reflection.—"I was witness to-day of a poisoning scene, which it would appear is a favourite punishment at Fundah. The culprits were two women, who were placed under a tree in the court-yard, and an old man beat up the leaves of some herbs in a sort of mortar, the women sitting quietly looking on. The liquid, which was of a greenish colour, was poured into two calabashes, and the women drank it off without any apparent reluctance. They then commenced walking up and down the court, drinking large quantities of water from a calabash placed in the centre

of it. In about half an hour they both began to stagger and totter in their walk; and in a few minutes more the tragedy was ended by their falling flat on the ground and expiring apparently in dreadful agonies.—*Narrative of an Expedition into the Interior of Africa.*

6. *Obesity in Africa.*—It was a subject of remark among us, and occasioned some amusement to see the different effects of heat on different constitutions. Sometimes, with the thermometer at 84°, I felt cold in a blanket dress, and at other times, at 75°, I was oppressed with heat; it appeared, however, to depend much on the moist or dry state of the atmosphere. I found that a very simple rule had hitherto kept me in excellent health; if I felt sleepy after a meal, I considered it a gentle hint from my stomach that I was over-working it, and reduced my fare accordingly. In fact, I thought the less one consumed the better, *as all our party appeared to have a most unaccountable propensity to become fat.* I did not eat one-half that I had been accustomed to do in England, and yet could not keep myself from increasing. Dr Briggs was precisely in the same way; and as for Lander, he was as broad as he was long!—*Ibid.*

Proceedings of the Society of Arts for Scotland.

List of Prizes for Session 1837–38.—The Society for the Encouragement of the Useful Arts in Scotland, announce the following Prizes for the Session 1837–38:—

1. For the most important Invention, Discovery, or Improvement in the useful Arts;—*The* KEITH *Medal, value Twenty Sovereigns.*
2. For the best series of Experiments applicable to the Useful Arts;—*The* SOCIETY'S *Silver Medal, value Ten Sovereigns.*
3. For the most important Communication of any useful Invention, Process, or Practice, from Foreign Countries, not yet known or adopted in Britain;—*The Silver Medal, value Five Sovereigns.*

For a convenient mode of filling the Boilers of Steam Vessels with water, while the Vessel is at rest, so as to remove a frequent cause of explosion;—*The Silver Medal, value Ten Sovereigns.*

5. For an improved Twyre for the Hot-Blast Furnaces used in Scotland for the Manufacture of Iron, so as to prevent the accidents arising from the defects of those at present in use; —*The Silver Medal, value Five Sovereigns.*
6. For a Portable Apparatus for the Manufacture of Gas, to be used in lighting Country Houses, or other limited Establishments;—*The Silver Medal, value Five Sovereigns.*
7. For a convenient and simple method of increasing or diminishing the distance of the Floats from the centre of the common Paddle-Wheels, during the motion of the Vessel, so as to adapt them to changes in the load and draft of water;—*The Silver Medal, value Five Sovereigns.*
8. For a method of removing the Plaster of Paris from the Types, after Stereotyping, without injuring the Types;—*The Silver Medal, value Five Sovereigns.*
9. For a mode of depriving the Mucilage of Fuci and Lichens of disagreeable taste and odour;—*The Silver Medal, value Five Sovereigns.*
10. For the best and simplest means of ascertaining the quantity of absolute Alcohol in any mixture;—*The Silver Medal.*
11. For an improvement in the methods used in Britain for Currying and Tawing certain kinds of Leather, so as to render them equal to those done in France;—*The Silver Medal.*
12. For a good Copying Ink, more fluent and permanent than the present;—*The Silver Medal.*
13. The Society will be ready to expend a farther sum in *various* Pecuniary Prizes, and Honorary Medals—in rewarding other Inventions and Improvements which may be submitted to them;—or Essays or detailed Accounts of Public or other Undertakings of great National importance, not previously published.

General Observations.—Communications lodged in competition for the specific Prizes, Nos. 4 to 12 inclusive, are not thereby prevented from competing for the KEITH *Medal.*

The Society eserve to themselves the power of determining whether any communication be of sufficient merit to entitle it to the Prize for which it competes, and of modifying the amount of the Prize.

The Society shall be at liberty to publish, in their Transactions, copies or abstracts of all Papers submitted to them. All Models, Drawings, &c., for which Prizes shall be given, shall be held to be the property of the Society; and these and all others which are approved of by the Society shall be entitled to a place in the MUSEUM.

The Society particularly request, that all communications intended for competition may be forwarded to the Secretary *as soon after 1st November* 1837 *as possible*, in order to ensure their being read and reported on during the Session. *But they will be received at any time till* 1st *April* 1838.

Communications, Models, &c., to be addressed and forwarded to the SECRETARY, at the MUSEUM of the SOCIETY OF ARTS, 63 Hanover Street, Edinburgh; and it is requested, that, in all cases, *full descriptions* of the Inventions may be sent; and, where the nature of the communication requires it, that there be also sent relative *Models*, *Drawings*, or *Sketches*, so as to enable the Society fully to judge of the merits of the communication.

ROYAL INSTITUTION, EDINBURGH, *5th July* 1837.

List of Patents granted in Scotland from 12*th June to* 20*th September* 1837, *inclusive.*

1. To PIERRE BARTLEMY GUINIBERT DEBAE, of Brixton, in the county of Surrey, civil engineer, for an invention of "improvements applicable to railroads."—12th June 1837.

2. To JOEL LIVSEY, of Bury, in the county of Lancaster, cotton-spinner, for an invention of "certain improvements in machinery used for spinning, preparing, and doubling cotton and other fibrous substances."—21st June 1837.

3. To ALEXANDER MACEWAN, grocer and tea-merchant in Glasgow, for an invention of "a process for the improvement of teas as ordinarily imported." —26th June 1837.

4. To JAMES LEONARD CLEMENT THOMAS, of Covent-Garden, in the county of Middlesex, Esq., for an invention, communicated by a foreigner residing abroad, of "an improvement applicable to steam-engines and steam-generators, having for its object economy of fuel."—7th July 1837.

5. To JOHN SPURGIN, of Guildford Street, Russell Square, in the county of Middlesex, Doctor of Medicine, for an invention of "an improvement or improvements in the mode or means of propelling vessels through the water, and part of which means may be applied to other useful purposes."—14th July 1837.

6. To GEORGE NELSON, of Leamington Priors, in the county of Warwick, entleman, for an invention of "a certain new or improved process or pro-

cesses, by the use of which the qualities of a certain gelatinous substance, or certain gelatinous substances, called isinglass, may be improved."—18th July 1837.

7. To **Thomas Lutwyche**, of Liverpool, in the county of Lancaster, manufacturing chemist, for an invention of "certain improvements in the construction of apparatus used in the decomposition of common salt, and in the mode or method of working or using the same."—20th July 1837.

8. To **William Bell**, of Edinburgh, in the kingdom of Scotland, Esq., for an invention of "improvements in heating and evaporating fluids."—21st July 1837.

9. To **James Dredge**, of the parish of Walcot, in the city of Bath, and county of Somerset, brewer, for an invention of "certain improvements in the construction of suspension-chains for bridges, viaducts, aqueducts, and other purposes, and in the construction of such bridges, viaducts, or aqueducts."—22d July 1837.

10. To **Godfrey Woone**, of Berkeley Street, Picadilly in the county of Middlesex, gentleman, for an invention of "an improved method of forming plates with raised surfaces thereon, for printing impressions on different substances."—24th July 1837.

11. To **Robert Griffiths**, of Smethwick, near Birmingham, in the county of Warwick, machine-maker, for an invention of "improvements in the manufacture of burrs or nuts for screws, and nails or spikes and bolts."—9th August 1837.

12. To **William Henry Goschen**, of Crosby Square, Bishopsgate Street, in the city of London, merchant, for an invention, communicated by a foreigner residing abroad, of "improvements in preparing flax and hemp for spinning."—9th August 1837.

13. To **John Paul Neumann**, of Great Tower Street, in the city of London, prussiate of potash-maker, for an invention, partly his own, and partly communicated by a foreigner residing abroad, of "improvements in the manufacture of prussiate of potash, and prussiate of soda."—9th August 1837.

14. To **Andrew Smith**, of Balper, in the county of Derby, mill-wright and engineer, for an invention of "a certain improvement or improvements in printing-machines."—17th August 1837.

15. To **Lemuel Wellman Wright**, of Sloan Terrace, in the parish of St Luke, Chelsea, and county of Middlesex, engineer, for an invention of "certain improvements in machinery or apparatus for bleaching and cleaning linens, cottons, and other fibrous substances."—28th August 1837.

16. To **Archibald Francis Richard Rosser**, of New Boswell Court, in the county of Middlesex, Esq. for an invention, communicated by a foreigner residing abroad, of "improvements in preparing manure, and in the cultivation of land."—1st September 1837.

17. To **John George Hartley**, of No. 11. Beaumont Row, Mile End Road, in the county of Middlesex, Esq. for an invention for "an improved application of levers for the purpose of multiplying power."—1st September 1837.

18. To **James Hunter**, of Leys Mill, Arbroath, in the county of Forfar,

chanic, for an invention of "a machine for boring or perforatingstones and
her substances."—4th September 1837.

19. To **Henry Stephens**, of Charlotte Street, in the parish of St Maryle-
one, in the county of Middlesex, gentleman, and **Ebenezer Nash**, of Bur-
ss Street, in the parish of Saint George in the East and county of Middle-
ex, tallow-chandler, for an invention of "certain improvements in manufac-
turing colouring matter, and rendering certain colour or colours applicable to
yeing, staining, and writing."—6th September 1837.

20. To **Thomas Hancock**, of Goswell Mews, Goswell Road in the county
f Middlesex, water proof cloth manufacturer, for an invention of "an im-
ovement or improvements in the process of rendering cloth and other fab-
s partially or entirely impervious to air and water, by means of Caoutchouc
Indian rubber."—8th September 1837.

21. To **Henry Vere Huntley**, of Great Russell Street in the county of
Middlesex, Lieutenant in the Royal Navy, for an invention of "improve-
ents in apparatus for facilitating the securing of ships' masts."—14th Sep-
nber 1837.

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Fig. 3. 4. Fossil Quadrumana.

Fig. 3.

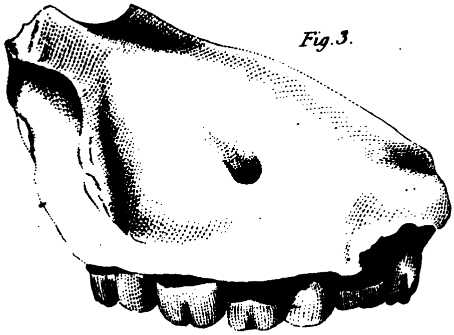


Fig. 4.

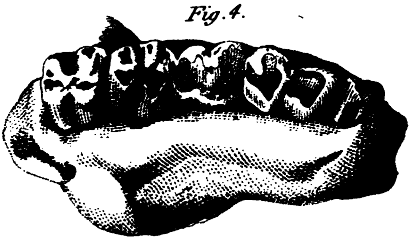


Fig. 2.

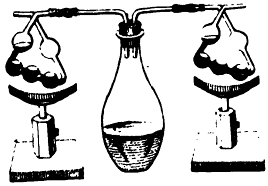
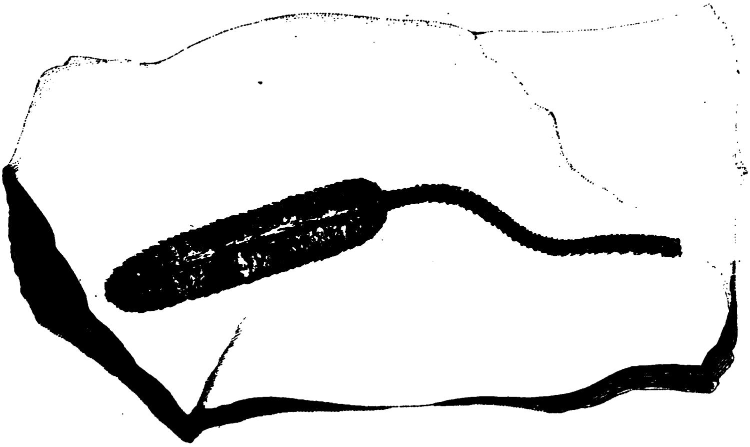
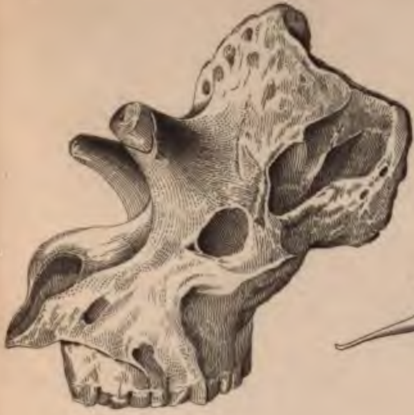


Fig. 1.

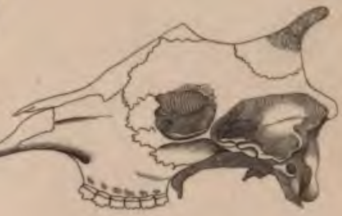


Lepidostrobus Variabilis
with attached Lepidodendron.

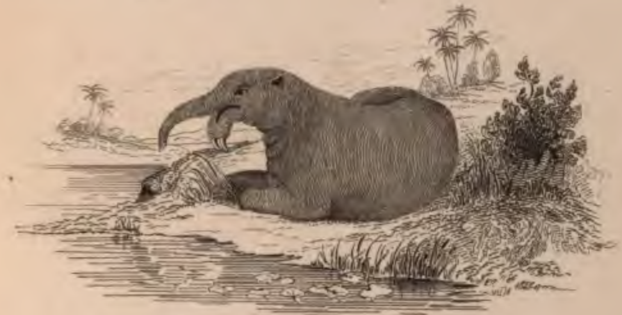
E. Mitchell. Sc.



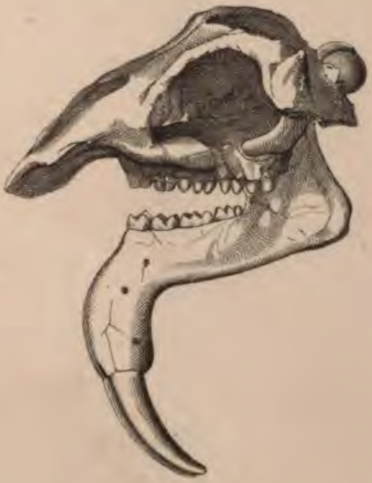
Sivatherium giganteum.



Giraffe.



Restored Dinatherium giganteum.



Dinatherium giganteum.

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